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RECENT STUDIES OF VOLCANIC ACTION.

By G. F. Rodwell.

ALTHOUGH, during the last few years, no great eruption of a European volcano, similar to that of Vesuvius in 1872, has occurred, and no great earthquake comparable with that which devastated Calabria in 1783, the records of minor volcanic disturbances have been unusually numerous. In 1878 Etna poured out a stream of lava which ran for six miles; a few months before, Hekla threw up a new monticule, from which a quantity of lava issued; Vesuvius more than once since 1872 has furnished enough lava to run into the Atrio del Cavallo; Santorin was active from 1866 to 1870; Stromboli is always more or less energetic; and Volcano has given evidence that its vitality is by no means dormant. Again, within the last year, three disastrous earthquakes have occurred in Europe—at Agram, in Ischia, and in Chios—and many minor shocks have been recorded. In fact, earthquakes are much more common than we generally realise. A year ago (Nov. 28, 1880) an earthquake was reported from no less than fifty different stations in Scotland; some days ago a severe shock was announced from Agram; the previous day from Switzerland. During a few months of 1879, earthquake shocks were felt in such widely-distant places as Florence, Aachen, North Wales, Carinthia, Agram, Athens, and Swizerland. In 1878 there were more than a hundred earthquakes and twelve volcanic eruptions.

For another reason it appears to be a not inappropriate time for reviewing recent European Vulcanology. The literature of the subject has lately received some important additions in the form of such books as Dr. Arnold von Lasaulx's "Der Ætna"; M. Fouquè's "Santorin et ses Éruptions"; the "Studien über Erdbeben" of Dr. Julius Schmidt; the United States' "Report of the Geology of Utah," by Captain Dutton; the "Minéralogie Micrographique" of MM. Fouquè and Michel Lèwy; and the treatise on "Volcanoes" of Professor J. W. Judd, which only within the last few months has issued from the ponderous and iron jaws of the press. The rapid growth of the literature of the subject may be fairly judged of from the fact that Von Lasaulx prints sixteen quarto pages of "Ætna-Literatur."

Vulcanology is a recent science. If any one man can originate a science, Spallanzani must be regarded as the father of vulcanology. There had been many observers before his time, for the great phenomena of nature receive the earnest attention even of unlettered men, but they had observed blindly, and without method. They recorded their observations, but they made no attempt to classify or correlate them, formed no hypothesis to embrace them, and did not follow up the particular line of thought suggested by one or other of them. The value of a good working hypothesis, even in the early stages of a science, cannot be overrated. Moreover, the earlier vulcanologists did not go to work in the right way; they either watched, necessarily at a distance, some grand paroxysmal outburst, or they visited the scene of its action when the main effects had died out. Spallanzani did otherwise. He chose as the source of his observations an ever-active volcano, which never approaches the paroxysmal violence of Etna, Hekla, and Vesuvius during their great eruptions, and never sinks to the quasi-dormant condition which a Volcano commonly presents. Stromboli has been active for more than 2,000 years, and so moderately active, that the scene of its operations can be closely approached, and from a projection which is situated a little above the crater, the observer may sit for hours when the steam and vapours are blown out to sea, and watch the phenomena which are taking place within the crater. Spallanzani did this; and for the first time realised and enunciated the important fact, that volcanic phenomena are mainly, if not entirely, due to the violent escape of steam and other gases at high pressure from molten matter.

In 1874, Professor J. W. Judd, already an accomplished disciple of Scrope, visited Stromboli, and minutely examined, from the vantage-ground erst occupied by Spallanzani, the operations taking place upon the floor of its crater. He divides them into three classes. From certain large apertures in the floor steam escaped in loud, irregular puffs; within, some lava could be seen, which at intervals rose and swelled out, at the same time emitting large volumes of steam; while within the depths of other openings, viscous molten matter was seen to be heaving up and down, and violently agitated, like boiling water. Ever and anon, as the agitation increased, a great bubble of lava would swell out, and suddenly burst, emitting steam at high pressure, the force of which hurled the red-hot scum high into the air. Thus, the three essential conditions for the production of volcanic phenomena appear to be:—(1) apertures or fissures affording communication with the interior of the earth; (2) highly-heated matter beneath the surface; and (3) subterranean water which, in the form of high-pressure steam, is competent to produce all the crater operations. A mass of lava within a small active crater precisely resembles a boiling fluid. In one of the lateral cones of Vesuvius the writer once saw a veritable geyser of fire, imitating in all respects its great prototype at Haukadalr. The viscous seething mass of lava within the *bocca* contains water entangled in its mass, and when this rises to the upper part of the column of lava, it is relieved from pressure, and flashes explosively into steam. This restores equilibrium for awhile, during which more steam is being generated, and presently another outburst occurs. The pressure of the steam which thus accumulates may be judged of by the fact that, during the eruption of Vesuvius in 1872, masses of vapour (and, it is said, fragments of scoriæ), were projected to a height of nearly four miles. The friction of this steam against the

rock masses generates enormous quantities of electricity, which appears as flashes of lightning issuing from the column of steam above the crater. The generation of the steam within the molten mass, and its competence to produce all volcanic phenomena, are ably discussed by Prof. Judd from a thoroughly scientific standpoint, and the result of the discussion he gives us in the cardinal generalisation—the keynote of modern vulcanology:—"The varied appearances presented, alike in the grandest and feeblest outbursts, can all be referred to one simple cause, viz., the escape from the midst of molten materials of imprisoned steam or water-gas."

Now the presence of large quantities of water within the recesses of the earth is by no means difficult to account for; but the cause of the internal heat is a far more difficult and yet unsolved problem. Astronomy has taught us that the world is not, as was long believed, a liquid mass, surrounded by a thin solid shell. Sir Humphry Davy endeavoured to account for the heat of volcanoes on the supposition that large quantities of uncombined alkali metals exist in the earth, which, when water finds its way beneath the earth, violently decompose it, generating heat and evolving hydrogen. Mr. Mallet considers that the contraction of the earth's crust can develope enormous quantities of heat. It has been calculated that if a portion of the earth's crust, fifty miles in thickness, were to have its temperature raised 200° Fahr., the surface would be raised by expansion more than a thousand feet. Moreover, crumpled and distorted strata clearly prove that enormous pressures have been exerted by contracting rock-masses. Hopkins imagines that the earth has solidified both at the centre and circumference, while cavities of molten matter are distributed between them. Captain Dutton believes that lava is pressed to the surface by the weight of superincumbent rocks, and he admits that we cannot allow one general reservoir, because lavas of different composition come from the same crater at different periods, and a lower volcanic vent sometimes remains open, while the lava rises and flows from a higher one. Some have assumed that the interior of the earth, although intensely heated, remains solid on account of the enormous pressure, and that the effect of any local diminution of pressure is to lower the fusing point, and thus cause the solid to become liquid. Volcanic phenomena would thus be exhibited at the points of diminished pressure.

But, although we seem to be as far as ever from determining the ultimate cause of volcanic effects, the phenomena themselves, as presented to our observation, are far better understood than heretofore; and this is mainly due to two causes, viz., the establishment of seismological observatories, and the microscopic study of eruptive rocks. Of course, we must add to these the application of more exact and scientific methods, and the advances due to the perfection of such instruments as the spectroscope and recording seismometer. The Vesuvius observatory was the first to be established on anything like a scientific basis, and several valuable volumes of reports have been published by its director, Palmieri. For sixteen years, Tacchini, of Palermo, and Silvestri, of Catania, have urged the erection of an observatory near the summit of Etna, and, thanks to the liberality of the Italian Minister of Agriculture and Commerce, and of the Municipality of Catania, it is now an accomplished fact. In August, 1882, the observatory will be ready for use. It adjoins the site of the old Casa Inglese, nearly 10,000 feet above the sea, and it is in telegraphic communication with an observatory in Catania, 24 ft. above the sea level, and also with observatories in the seven principal towns upon the flanks of the mountain —Aci Reale, Randazzo, Paterno, Aderno, Bronte, Giarre, and Linguaglossa. It will be furnished with all the most approved meteorological and seismological instruments, with spectroscopes, and with a fine refractor by Merz, of Munich, the object glass of which, during the winter months, will be transported to the Catania observatory and mounted in a similar telescope. Owing to the persevering efforts of Prof. M. S. di Rossi, of Rome, who is the editor of the *Bulletino del Vulcanismo Italiano*, seismological observations are now made in more than fifty towns of Italy, and although we could wish that they were better organised and placed under the control of some central Government observatory in Rome, it cannot be denied that the *Bulletino* (now in its seventh year of publication) has placed on record a multitude of observations of high interest to vulcanologists. Observatories are now being instituted in very distant volcanic centres. In January last, Lord Granville forwarded to the Royal Society some valuable "notes on the earthquake of July, 1880, at Manila," by Commander W. B. Pauli, which record the first scientific account of an earthquake in the Philippine Islands. It occurred along the line of the Taal volcanoes, and affected an area 220 miles by 75. The most severe shock lasted for 70 seconds, and combined oscillatory, trepidatory, and rotatory movement. Some very interesting engravings of pendulum curves accompany the paper, which appears in the *Proceedings of the Royal Society* for February, 1881. The curves are of great complexity, and show both the direction and relative intensity of the earthquake shocks, which were sometimes so violent that the pendulum was jerked upwards from the paper, and thus broke the continuity of the curve. Seismological instruments, although far from perfect, have been much improved since the time when the patient student of vulcanology contented himself with watching a bowl of treacle. The electrical seismograph records the direction of the shock, its intensity, and the moment of its occurrence and its cessation. Professor M. S. di Rossi has ingeniously applied the microphone to the detection of the slight subterranean noises which may be heard at any hour of the day or night in some districts. He affirms that both in his observatory in Rome, near the Ara Cœli, and at Rocca di Papa, he has often, while watching the point of a seismic pendulum in a microscope and simultaneouly applying the telephone to his ear, heard harsh sounds in the latter at the instant when the pendulum has been seen to quiver.

DREAMS.

PART II.

By Edward Clodd.

KEEPING in mind what has been said about savage mental philosophy, it is not surprising that the inference drawn from the phenomena of dreams is belief in a double existence. Besides that waking self of which the savage is hazily conscious, there must be *another self*, which, roaming the world while the body is at rest, sees and does the things dreamed. Waking, the savage knows, or will be told, that whatever his dreams reveal to the contrary, he has not moved from the place where he lay down; therefore it is that ghost-soul—that *other* self—which has been away on the strange or familiar errand. And such belief in another self—*in* the body, yet at times not *of* it—is confirmed by daily experience. There are the suspensions of consciousness witnessed in swoon, apoplexy, catalepsy, and other forms of insensibility. Then there are the phenomena of shadows and reflection, actual existences to the savage, mocking doubles of himself. The shadow

accompanies, goes before, or follows him by sunlight and by moonshine, disappearing mysteriously only when these are withdrawn or intercepted. Still more complete in its mimicry is the reflection of himself—the image repeating every gesture, while perchance, as he stands shouting by the stream, the echo of his voice is thrown back from the hill-side, and adds confirmation to his notion of duality. How else can man at low stages of thinking, ignorant of the laws that govern the reflection of both sound and light, interpret the shadow and the echo? Hence it is that we find the word for "shadow" chosen to express this other-self in both barbaric and civilised speech, from the dialects of North and South American and African tribes, to the classic and modern languages, as witness the *skia* of the Greeks, the *manes* or *umbra* of the Romans, and the *shade* of our own tongue. Did the limits of a brief paper allow, it would be easy to show, from the evidence of language, how man explained to himself the mode in which this other self makes the passage from the body to the external world, and wherein lay the difference between the sleeping and waking, the living and lifeless body. It must suffice to say that throughout the entire savage and civilized world, the life, the spirit, the soul of man has been identified with *breath*. Not with that alone; but with the blood, the heart, &c., although chiefly and universally with the act of breathing, "so characteristic of the higher animals during life, and coinciding so closely with life in its departure."

It is interesting to watch the primitive nebulous theories of another self, a vaporous, ethereal, or otherwise unsubstantial, impalpable thing, condensing into theories of semi-substantiality, or of rude or refined resemblance to the body, theories which become indispensable to account for the appearance of both the living and the dead in dreams, when their persons were clasped, their forms and faces seen, their voices heard.

Such theories differ not in kind, but only in degree of refinement, and unite, as Dr. Tylor remarks, "in an unbroken line of mental connection," the savage fetish worshipper and the civilised psychologist adding their welcome witness to the similar working of untrained intelligence in different ages among different races on corresponding levels of culture, and therefore, to the underlying unity of our race. This we shall realise only as we realise that the laws of mind, like those of matter, are uniform, and approximately calculable in their operation; the phenomena of one interrelated and interdependent as are the phenomena of the other, and equally the subjects of observation and comparison, if not by identical methods, yet on like principles.

It would be an interesting and informing chapter in the history of the illusions through which man has made continuous, and as yet unaccomplished, passage to the truth, to show how belief in indwelling spirits, of fitful habit and varying form, was enlarged to belief in souls in the lower animals, in plants, and in lifeless things, from stars to stones; how the phantasms of the brain have filled earth, sea, and sky with spirits innumerable, from white-winged celestials to the degraded ghosts of haunted houses. But this would be an undue extension of the subject, for the completeness of which some reference must be made to the part played by dreams as supposed media of communication between gods and men, and as monitions of coming events.

The awe and wonder excited in the savage mind by waving trees and swirling waters, by drifting cloud, whistling wind, and stately march of sun and moon—all invested by him with personal life and will—were immensely quickened by his dreams. In their unrelated and bewildering incidents, the powers indwelling in all things around him seemed to come nearer than in the more monotonous events of the day, uttering their warnings and conveying their messages. There needed but slender data to reach conclusions. Let the death of a friend be dreamt of, and the event follow; or a hunting-feast fill the half-torpid fancy, and a day's privation give the lie to the dream; the arbitrary relation is made. Lord Bacon says:—"Men mark the hits, but not the misses," and a thousand dreams unfulfilled count as nothing against one dream fulfilled. Out of that a canon of interpretation is framed by which whole races of men will explain their dreams, never staying to wonder that the correspondences are not more frequent and minute than they really are.

"To this delusion," says Cornelius Agrippa, an ancient rationalist, "not a few great philosophers have given a little credit . . . so far building upon examples of dreams, which some accident hath made to be true, that thence they endeavour to persuade men that there are no dreams but what are real." When Homer says that "dreams, too, from Jove proceed,"* painting the vividness and agonising incompleteness of those passing visions; when Tertullian says that "we receive dreams from God, there being no man so foolish as never to have known any dreams come true," both classic and patristic opinion are clearly survivals from the lower culture, its lineal and thinly-disguised descendants. For the savage, the bard, and the theologian lived in days when the conception of orderly sequence was unthinkable to them; where the arbitrary act was wrought, the isolated or the conflicting influence manifest, there the deity or the devil was present; while for us, could we discover where law is not, thence God would seem to have withdrawn.

The passage from the crude interpretation of his dreams by the savage to the formal elaboration of the dream-oracle is obvious, the more so as this latter was only one of many modes by which it was sought to divine the will of heaven, and read that "book of fate" hidden from men. This dream-lore, as ancient records far back to Accadian times show, not only called into existence a class of men whose position as interpreters of royal and other dreams ensured them commanding place, but gave rise to a mass of literature most prolific in classic times. It maintained an almost canonical supremacy down to the Middle Ages, finding its befitting level in our day in the "Libri dei Sogni" which the Italian lottery-gambler consults, and in the "Imperial Dream-Book" by which the English domestic forecasts whether King Cophetua or Police-sergeant X 32 is to be her fate!

At this nether depth, Science, content with having shown the persistence of primitive modes of thinking in all subsequent interpretation of his own nature by man; finding its evidence and the warrant of its conclusions in that human experience which the sources of our knowledge cannot transcend; may well let the matter rest. It need not concern itself with denials that dreams have been sent as warnings from Heaven to man; this were as foolish as to take pains to disprove the existence of ghosts, or to seriously challenge the predictions in Zadkiel's *Vox Stellarum*. Science need not argue; it explains; and to such matters explanation is death. For the changes which revelation of the order of nature and the establishment of that doctrine of continuity, which has no "favoured-nation" clause for man, involve, will bring about, in quiet and unmourned, the departure of belief in dreams as omens or warnings, just as they have brought about the decay of belief in witchcraft and astrology.

* "Iliad," Book I., 77.

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BETTING AND MATHEMATICS.

By the Editor.

WHEN I was travelling in Australasia, I saw a good deal of a class of men with which, in this country, only betting men are likely to come much in contact—bookmakers, or men who make a profession of betting. What struck me most, perhaps, at first was, that they regarded their business as a distinct profession. Just as a man would say in England, I am a lawyer or a doctor, so these men would say that they were bookmakers. Yet, on consideration, I saw that there was nothing altogether novel in this. Others, whose business really is to gain money by making use of the weaknesses of their fellow-men, have not scrupled to call their employment a trade or a profession. Madame Rachel might have even raised her special occupation to the dignity of "a mystery" on Shakespearean grounds ("Painting, sir, I have heard say is a mystery, and members of my occupation using painting, do prove my occupation a mystery"); and if aught of wrong in his employment could be made out to the satisfaction of a bookmaker, his answer might be Shakespearean also, "Other sorts offend as well as we—ay, and bettor, too."

My own views about betting and bookmaking are regarded by many as unduly harsh, though I have admitted that the immorality which I find in betting has no existence with those who have not weighed the considerations on which a just opinion is based. I regard betting as essentially immoral so soon as its true nature is recognised. When a wager is made, and when after it has been lost and won its conditions are fulfilled, money has passed from one person to another without any "work done" by which society is benefited. The feeling underlying the transaction has been greed of gain, however disguised as merely strong advocacy of some opinion—an opinion, perhaps, as to whether some horse will run a certain distance faster than another, whether certain dice will show a greater or less number of points, or the like. If here and there some few are to be found so strangely constituted mentally as really to take interest in having correct opinions on such matters, they are so few that they do not affect the general conclusion. They may bet to show they really think in such and such a way, and not to win money; but the great majority of betting men, professional (save the mark) or otherwise, want to win money (which is right enough), and to win money without working or doing some good for it, which is essentially immoral. That in a very large proportion of cases this negative immorality assumes a positive form—men trying to make unfair wagers (by betting with unfair knowledge of the real chances)—no one acquainted with the betting world, no one who reads a sporting paper, no one even who reads the sporting columns of the daily papers, can fail to see. Why, if half the assurances of the various sporting prophets were trustworthy, betting, assisted by their instructions, would be as dishonourable as gambling with marked cards, as dishonest as picking pockets. Here is my "Vaticinator,"* the betting man might say, who says that Roguery is almost sure to win the "Beggar my Neighbour" stakes, but if he does not, that speedy mare, Rascality, will unquestionably win. Here are the bookmakers, who seem all quite as ready to lay the odds against Roguery and Rascality as against any of the other horses, to say nothing of my friends, Verdant and Flathead, who will freely back any of these latter. Now, if I back Roguery and Rascality with the bookmakers, and lay odds against the certain losers in the race, I shall certainly win all round. Of course, "Vaticinator" is not the prophet he claims to be, but the betting-man of our soliloquy supposes that he is, and so far as the morality of the course the latter follows is concerned, the case is the same as though "Vaticinator's" prophecies were gospel. There is not a particle of real distinction between what the bettor wants to do, and what a gambler, with cogged dice or marked cards, actually does. The more knowing a betting man claims to be, the easier it is to see that he wants and expects to take unfair advantage of other men. Either he knows more than those he bets with about the real conditions of the race or contest on which they wager, or he does not. If he does, he wagers with them unfairly, and might as well pick their pockets. If he does not, but fancies he does, he is as dishonest in intention as he is in the former case in reality. If he does not, and knows he does not, he simply lies in claiming to know more than he does. In claiming to be knowing, he really claims to be dishonest and (which is not quite the same thing) dishonourable; and probably his claim is just. Of course, this is only a comparatively mild case. Men have been known to take the odds against a horse after they knew certainly that the horse would not run. Others, a shade more advanced, have been known to bribe a jockey to "hold," or "rope" a horse, or a stableman to poison or stupefy him. Owners—aye, even "noble" owners—have been known to work the market in ways fully as flagitious. Every one agrees about these. But the majority are disposed to stare, and perhaps to sneer, when Herbert Spencer describes ordinary, and what is commonly called fair gambling, as immoral; and the calmness with which a betting man claims to be knowing, shows that he, at any rate, does not think wagering with unfair knowledge (with any knowledge, I suppose, short of absolute certainty) dishonourable. He argues, and many who do not bet argue for him, that he takes his chance with others; as if it might not quite as justly be argued that the pickpocket takes his chance between a successful transaction and the prison cell.

As one of our "Five of Clubs," I gave last week a case in which a certain man of title used to offer freely—and possibly with a sense that he was acting quite fairly—a most unfair wager, though it seemed a very generous one. Odds of a thousand pounds to one are very tempting to the inexperienced. "I risk my pound," such a one will say, "but no more, and I may win a thousand." That is the *chance;* and what is the *certainty?* The certainty is that in the long run such bets will involve a loss of £1,828 for each thousand pounds gained, or a net loss of £828. As certain to all intents as that two and two make four, a large number of wagers made on this plan would mean for the clever layer of the odds a very large gain. Yet Lord Yarborough would probably have been indignant to a degree if he had been told that in taking £1 for each hand on which he wagered which did not prove to be a "Yarborough," he was in truth defrauding the holder of the hand of 9s. 0¾d.—notwithstanding the preliminary agreement, simply because the preliminary agreement was an unfair one. As to his being told that even if he had wagered £1,828 against £1 the transaction would have been intrinsically immoral, doubtless he and his opponent would equally have scouted the idea.

A curious instance of the loss of all sense of honour, or even honesty, which betting begets, occurred to me when I was in New Zealand. A bookmaker ("by profession," as

* I hope there is no turf prophet with this *nom-de-plume*. I know of none, or I would not use the name; but it may have been hit upon by some sporting man with a taste for polysyllables.

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he said), as genial and good-natured a man as one would care to meet, and with a strong sense of right and justice outside betting, had learned somehow that ten horses can come in (apart from dead heats) in 3,628,800 different ways. This curious piece of information seemed to him an admirable way of gaining money from the inexperienced. So he began to wager about it, endeavouring—though, as will be seen, he failed—to win money by wagering on a certainty. Unfortunately, he came early across a man as cute as himself, and a shade cuter (*à brigand brigand et demi*), who worded the question on which the wager turns, thus: "In how many ways can ten horses be placed?" Of course, this is a very different thing. Only the first three horses can be placed, and the sets of three which can be made out of ten horses number only 10 times 9 times 8, or 720 (there are only 120 actual sets of three, but each set can be placed in six different ways). My genial, but (whatever he thought himself) not quite honest friend, submitted the matter to me. Not noticing, at first, the technical use of the word "placed," I told him there were 3,628,800 different arrangements, he rejoiced as though the money wagered were already in his pocket. When this was corrected, and I told him his opponent had certainly won, as the question would be understood by betting men, he was at first depressed; but presently recovering, he said, "Ah, well; I shall win more out of this little trick, now I see through it, than I lose this time."

I shall hereafter give some illustrations of the true principles on which all chance questions should be determined. There is no hope that men generally will give up gambling, but it is, at any rate, desirable that when they gamble, the chances should be as equal as they can be made; that, in fact, they should not play (as the opponents of Lord Yarborough and my New Zealand friend were certainly playing) against cogged dice or marked cards. The matter is one strictly appertaining to the subjects with which KNOWLEDGE claims to deal. There is science in chance, certainty in probabilities. What is thus scientific and certain is what we propose to bring before our readers.

OUR UNBIDDEN GUESTS.

By Dr. Andrew Wilson, F.R.S.E.

THE fact that in most animals there may reside, as "guests," within unconscious or unwilling "hosts," certain other animal forms, is, of course, widely known. These animal "guests" form the "parasites" of the natural historian. But, although the fact of their existence is known, the general history of even the commonest parasites is a matter concerning which the general public are, as a rule, lamentably ignorant. I say "lamentably," and I mean what I say. A vast amount of disease, and that of a preventible nature, is caused by the carelessness of man in the preparation of his food. This carelessness is in its turn founded upon gross ignorance, for there are not a few persons who believe that parasites come, like Dogberry's reading and writing, by nature, and that they are part and parcel of an animal's constitution. That this opinion is very far removed from the true state of matters can easily be shown. It is perfectly provable that animals were not created with the parasites infesting them as we find them to-day. Common-sense forbids such a supposition, and the organised common-sense we call "science" shows us that the reverse is the case. All parasites are acquired, and not original "guests." This alone is provable by the facts of parasite-development. There is a bag-like parasite called *Sacculina*, for instance, which attaches itself to the bodies of hermit crabs. Now, sac-like though this parasite is, and destitute as it is of all the ordinary belongings of animal life, it yet begins its existence as a little free-swimming animal, exactly resembling a water-flea. The first stages in a sacculina's development are, in short, like the beginnings of the development of some shrimps, of barnacles, of water-fleas, and of crabs themselves, though in a less marked degree. Only after becoming degraded in structure does the sacculina become the "guest" of the crab. The mere facts that sacculina is at first as free-living as a fish, and that it afterwards settles down on the crab, testify, if we read nature's story aright, that "once upon a time" the sacculina race was not a parasitic one. Whether or not the sacculina-stage itself was the beginning of the attached existence, we do not know. It is most probable that the bag-like body we term a "sacculina" was the result of the adoption of the lower and rooted way of life. But, apart from all other considerations, the main facts that a young sacculina is always free, and that it begins life under a similar guise even to some of the shrimp race, shows that its parasitic life has been acquired, and is by no means an original condition.

Now the same rule holds good of all "parasites." The development of most of them shows us the lingering remnants of a once-free life. But there are other proofs at hand of this assertion. There are degrees and stages in the perfection of the parasitic state. There exist animals which are mere "lodgers," so to speak—who "dine out," but who repose within the anatomical establishment of a "host." This is the case with certain little fishes, which choose the very "jaws of the lion" as a dwelling-place, since they appear to live in the interior of certain big, tropical sea-anemones. These fishes may be seen to swim in and out of the anemone's mouth, and they may be enclosed within the anemone's body when that animal contracts itself, and yet swim free and unharmed out of the mouth when these flower-like animals once more resume their normal and expanded state. Here, then, there is mere "association," but it is in some such association that the beginnings of pure parasitism have orginated. Suppose the case of an animal which, at first merely "lodger," took to feeding upon the tit-bits secured by its host for home-consumption. The "lodger," in such a case, would practically become a "boarder" as well. But nature has a law as fixed as the edicts of the Medes and Persians, called the "law of disuse." This law enacts that whatever structures or organs of living beings are not normally used, will waste and tend to disappear. It is the operation of this law which has caused the two outer toes of our horse to grow "small by degrees and beautifully less," until they now appear as the "splint bones" on each side of the single toe upon which the horse walks. And applying this law to the case of the animal lodger, we see how an animal which does not require to move about when resident within another animal will lose its organs of motion. If it obtains fluid food, all ready digested, the probabilities are its digestive system will become rudimentary. Not requiring eyes or other sense-organs, these will disappear; and thus we see represented a kind of zoological backsliding, which reduces the parasite to the elementary and degraded condition we, as a rule, discover in the races of animal "guests."

The histories of some of the most common parasites are fraught with instruction, not to speak of the curiosity that invests them. Take, for instance, the history of the fluke (*Fasciola hepatica*), found in the bile-ducts of the liver of the sheep and ox. It is the presence of this parasite that makes

sheep fall into a decline, known to veterinarians as the "rot." A fluke is a little, flattened, oval body, about 1 in. or ¾ in. in length, and about ½ in. in breadth. It possesses a nervous system, a set of water vessels, two suckers, a branched digestive system, and an egg-producing apparatus. It has no organs of motion, but it is by no means a very degraded being after all. Its development is very curious. The eggs, liberated from the animal "host," get scattered abroad. Many—as in the case of all parasites—must perish, but a proportion finding their way into water, enter the body of the water-snail, where they develop into curious little tailed beings called *Cercariæ;* and there are sundry other forms assumed by the fluke in the days of its youth, but which need not be mentioned here. Sooner or later, however, these *Cercariæ* escape into the water or into the meadows; and it is believed that from the damp meadows, or from the water itself, the sheep obtains these little beings. Once within the sheep's stomach, each *Cercaria* seems to waken up to its ultimate destiny. It drops its tail, and bores its way through the tissues of the sheep towards the liver, where it soon appears as the young fluke, which will develope eggs that will repeat its own curious history. The most notable fact, however, of this development is that if a sheep swallowed the *egg* of the fluke, no development would ensue. The egg requires to pass through its water-snail stage, ere the sheep can obtain the new fluke.

STATISTICS OF SUICIDE.*

IN these days, when the question is asked, as a not unreasonable comment on the phenomena of social life, "Is life worth living," the statistics of self-slaughter have an exceptional value. Suicide could not be regarded as a subject of scientific investigation at all, until after statistical researches had been made. As Professor Morselli says, the character which classical paganism attributed to suicide was simply individual. The famous phrase of the Stoics, "Mori licet cui vivere non placet" (he whom life pleases not has a free right to die), was the product of ancient philosophical individualism. Law and religion alike declare suicide criminal. But as yet the crime has not been considered as a tendency—hurtful, unquestionably, to society, but connected with society's natural development. It is this aspect of suicide which Professor Morselli discusses in the book before us. "The old philosophy of individualism," he remarks, "had given to suicide the character of liberty and spontaneity, but now it has become necessary to study it no longer as the expression of individual and independent faculties, but as a social phenomenon allied with all other racial forces."

At the outset, we may notice that if there is one thing which would render the statistics of suicide pre-eminently valuable, and if there is one thing which Dr. Morselli seems to regard as little worthy of discussion, it is the recognition of the motives which lead to suicide (and rather in their psychical than their social aspect). Can we, or can we not, from the statistics of suicide, determine the motives most potent to drive men to self-slaughter? By studying the statistics of times and seasons we may determine the physical condition which best favours the influence of such and such motives. When we find that the hot, bright months of summer are those in which the suicidal tendency prevails most, we seem to recognise physical, rather than psychical influences; or, to speak plainly, we seem to see that the state of the body, rather than that of the mind, is important, so far as this special peculiarity is concerned. When we learn that suicide is more common in the daytime than at night, and that there are three hours of maximum suicidal tendency—viz., about 8 a.m., about noon, and about 3 p.m.—we recognise the influence of social relations; as we do again when we notice the greater number of suicides on Monday, Tuesday, Wednesday, and Thursday, as compared with those on Friday, Saturday, and Sunday (except among women, with whom Sunday is the favourite day for suicide). But in these statistics the influence of motive is not readily to be recognised.

The case is somewhat different when we consider the influence of marriage on suicide. Here it comes out very clearly, as we might naturally expect, that responsibility has its influence even on those so weak that, but for some such restraining influence, they would "shuffle off this mortal coil." When in one and the same nation we find that the number of suicides among married men is far less than among single men, we may be in doubt how far the difference is due to difference of motive, or to the different stuff of which (taking the average) the ranks of the married and of the unmarried are made. It might be that the men who are either less likely to be selected as desirable mates, or who are averse to marriage, are more likely antecedentiy to be life-weary. But we can have no such doubts when we see that widowers without children are nearly twice as likely to commit suicide as widowers with children. It seems almost certain here that the restraining influence is the sense of responsibility. As Douglas Jerrold, when the doctors pronounced his death-warrant, looked round at wife and children, and said, "I will *not* die"; so we may well believe that even the weakest among men, who, on his own account, sees nothing that makes "life worth living," will yet feel that he cannot die and leave his little ones without a protector, or, it may be, in want. He feels that though his life may be worthless to himself, it is worth something still for them, and haply he may find later that in its worth to them it has had a worth to himself also.

We take it, indeed, that ninety-nine self-slayers out of a hundred show by their act that they feel their life to be worthless not only to themselves but to everyone. Suicide is, to all intents and purposes, an admission of utter worthlessness. Even in cases where a sort of halo of romance or heroism has surrounded the act, this has been so (we except, of course, all cases in which suicide has meant self-sacrifice—that is, no more self-murder than homicide in self-defence or defence of others is murder or even manslaughter). The lover who kills himself or herself because rejected or slighted, admits inferiority, if not worthlessness, as certainly as the man who kills himself because he has failed in the struggle of life. The Cato who kills himself rather than yield to his country's enemies, admits as certainly that his best has proved a failure, as the man who takes away his own life because he fears poverty or misery. Thus viewing suicide, one might, at a venture, predict that, other things being equal, suicide would be most frequent among that set of men who most feared contempt. Other things are not equal; but it does so happen that the proportion of suicides is greater in Paris (and the *Ile de France* generally) than anywhere else in Europe, four times greater than in London and the south-eastern parts of England, and nearly ten times greater than among the self-contented people of Southern Holland.

When we take employments, we find a similar lesson, though we cannot be quite so sure of our interpretation.

* "Suicide: An Essay on Comparative Moral Statistics." By Henry Morselli, M.D., Professor of Psychological Medicine, &c. (London: C. Kegan Paul & Co., 1881.) Price, 5s.

Men of art, science, and literature, with their keen susceptibilities, and their tendency to watch with anxiety, if not with envy, the success of their fellow-workers, stand at the head of the professions and trades in suicidal tendencies, despite the enormous increase to the value of life which the study of art, science, and literature brings with it. Military men come next, in Italy at least (with 404 to the million of their class, as compared with 618 to the million among literary men). But this may be due to other causes, as Dr. Morselli notes. "Military life" (in Italy), he says, "has the misfortune of increasing the loss of active and vigorous elements by means of unhappy sacrifice to suicide; whether that is owing to distance from home and disgust for military life, or to the severity of discipline, this is not the place to discuss."

Dr. Morselli's work is full of curious statistics, singularly ill-arranged, and at times rendered almost unmeaning for want of fuller information, or by the badness of the translation, yet well worth careful study. The conclusion to which he comes is melancholy. The cure of the suicidal tendency is indicated, he says, in one precept: "*To develope in man the power of well-ordering sentiments and ideas by which to reach a certain aim in life; in short, to give force and energy to the moral character.*" This amounts, in fact, to saying that, since the weak and idle are more apt to commit suicide than the strong and active, it is necessary to become strong and energetic in order to avoid the suicidal tendency. *But how?* "Intemperance and dissoluteness are powerful causes of weakness, and consequently of suicide." Therefore we must avoid intemperance and dissoluteness. But may it not with as much truth be said that weakness is a cause of intemperance and dissoluteness? We are no nearer the *causa causarum*—no nearer than was Hamlet when he reasoned how

> Oft it chances in particular men,
> That for some vicious mole of Nature in them,
> As in their birth (wherein they are not guilty,
> Since nature cannot choose his origin)
> By their [query "the"] off-growth of some complexion,
> Oft breaking down the pales and forts of reason
> Their virtues else . . . . .
> Shall in the general censure take corruption
> From that particular fault.

The true cure, it seems to us, is from without, not from within. Show a man that his life need not be a useless one; give him some worthy end to achieve, and his mind will be turned from the idle question: "Is life worth living?" Life is always worth living when any good work remains to be done.

THE DESTROYED COMET.

By the Editor.

BEFORE considering the theory of repulsion as applied to interpret the phenomena of comets' tails, it may be well to consider a case in which some active force (other than gravity), exerted by the sun, seems to have wrought the destruction of a comet, or, at least, to have broken up the comet into unrecognisable fragments.

No comet ever observed has exhibited phenomena more remarkable than those displayed by the comet known as Biela's (more properly called Gambart's). We wish we could agree with a modern astronomer, who has said that no comet has thrown more light on the nature of these bodies; but, in point of fact, it is only promise of light, not light itself, that we have obtained.

Discovered in 1826, Biela's comet was presently found to be identical with one seen in 1772 by Montaigne, and again by Pons in 1805. A careful study of the observations showed that the comet travels round the sun in a period of about 6¾ years, or, roughly, thrice in twenty years. Its path was found to approach very near to the path of our earth. The comet returned in 1832, when the ignorant were scared much as they have been recently by the threatened influence of the larger planets in perihelion. The comet crossed the earth's track several weeks before she herself came to the place where the two orbits approach nearest, and it is hardly necessary to say that the comet's passage did not injure the earth's roadway in any appreciable degree.

In 1839 the comet returned, but was not seen, travelling across a part of the heavens only above the horizon in the day-time, so that the comet's light was hidden by the sun's.

It was at the next return in 1845-46 that the comet first attracted special attention. On that occasion, instead of behaving as comets usually do, Biela's, which in the first days of 1846 had presented the appearance shown in Fig. 1, was found to have divided into two. There is some little doubt as to the time when the comet underwent division. Lieut. Maury reported on January 15 that he had seen the

Fig. 1.—Biela's Comet in 1846, before its division into two.

Fig. 2.—Biela's Comet on January 15, after its division into two.

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comet double on January 13; but Wichmann observed it as a single comet on the 16th. But Professor Challis, in his account of his own observations on the comets, states that even on January 15 the second comet might easily have been overlooked. M. Valz saw nothing unusual on the 18th and 20th; but on the 27th: "I was struck with amazement," he says, "to find two nebulosities, separated by an interval of two minutes of arc, instead of one nebulosity alone. . . . . Each head was followed by a short tail, whose direction was perpendicular to the line joining the two nebulosities." Earlier, only the larger comet had had a tail, the appearance presented by the double comet being that shown in Fig. 2.

The two comets travelled along, side by side, until at last both passed out of view, at which time the distance between them amounted to about 157,000 miles.

In 1852 both comets returned. Sir John Herschel says, in his "Familiar Lectures on Scientific Subjects," that when they returned, the distance between them was unchanged. This, however, was a mistake. The distance now amounted to about $1\frac{1}{4}$ millions of miles. Again they passed before the interested gaze of astronomers, travelling side by side, though rather far apart, until finally they disappeared from view—we say finally, for neither has ever been seen again.

Whether the two comets returned in 1859 is doubtful. It is certain that if they did, they would have been invisible, for the same reason that the comet was invisible when it returned in 1839.

But in 1866 the double comet should have been well seen. It should be remembered that each return of a comet of short period (like that which our correspondent Mr. F. Denning, of Bristol, discovered this year) gives the astronomer more perfect mastery of the comet's motions. The return could be predicted with sufficient accuracy in 1832 to cause the comet to be easily redetected. The next visible return might have involved a difficulty, because the comet had in the interval made two circuits. But that return was successfully predicted. The return in 1845-46 was still more accurately calculated. Nor did the breaking up of the comet into two on that occasion interfere with the successful calculation of the return in 1852. The case may be compared to the rating of a clock, which is more satisfactorily effected in a week than in a day, for the simple reason that any error of observation is spread in one case over seven times as long a period as in the other, and therefore affects the estimate of any given circuit of the hands by an error only one-seventh as large. Just so, whatever error an astronomer might make in observing Biela's comet in, say, 1843, was distributed over all the revolutions of the comet which had taken place since 1826 (one might almost say since 1772), and in a correspondingly small degree affected the astronomer's estimates of the comet's motion during any single revolution. This being so, astronomers had good reason for believing that in 1866 Biela's comet would return. When the time came that it should have been visible, telescopes were turned towards the spot where it should have been seen. Night after night from that time its calculated track was swept with the finest telescopes in Europe and America. But no trace of the comet could be seen. "It is now," wrote Sir John Herschel, in February, 1866, "overdue. Its orbit has been recomputed, and an ephemeris calculated. Astronomers have been eagerly looking out for its reappearance for the last two months, when, according to all former experience, it ought to have been conspicuously visible, but without success! giving rise to the strangest surmises. At all events, it seems to have fairly disappeared, and that without any such excuse as in the case of Lexell's—the preponderant attraction of some great planet. Can it have come into contact, or exceedingly close approach to some asteroid as yet undiscovered; or, peradventure, plunged into and got bewildered among the ring of meteorolites, which astronomers more than suspect?"

Be the cause what it might, the comet was not seen in 1866. In 1872 it was looked for even more carefully. Every possible contingency depending on planetary perturbations was considered; and the telescopes of astronomers swept, not only the calculated path, but to a considerable distance on either side of it. No trace of the comet was seen, however, in 1872 any more than in 1866. So far as telescopic observation is concerned, Biela's comet seems to have come to the end of its career as a comet.

Yet the observations of 1852 were not the last which were made on this interesting object. It has been seen again, though not as a comet. Nay, the occasion on which it was seen in the way referred to was predicted, and the prediction fulfilled, even in details. We shall return to the consideration of this remarkable apparition of the comet in changed form—a form which but a quarter of a century ago no one would have thought of associating in any way with the long-tailed star whose approach had been regarded as heralding some great change in the fortunes of men and nations.

TOAD IN A HOLE.

By Dr. A. Wilson, F.R.S.E.

In letter 87, "Arachnida" asks, "What is the construction of the common toad that enables it to be enclosed for many years in blocks of solid matter?" "Arachnida" should first of all have asked, is it true that toads are ever found so enclosed? The usual story is that of some quarrymen, who, blasting stones, see a live frog or toad hopping about, after the blast, among the *débris*. Because the toad is found thus, it is *assumed* that it came from the interior of the rock. Not a particle of evidence exists to show in such a case that the animal had anything whatever to do with the rock. If "Arachnida" will read in the "English Cyclopædia" the account of Dean Buckland's experiments, he will find that the Dean enclosed healthy frogs and toads in holes cut in limestone and sandstone blocks. He buried the blocks in his garden three feet deep. At the end of the first year most had died, and the living ones, re-buried, all died long before the end of the second year. Common sense, apart from exact knowledge, would tell us that animal life of higher kind, with all its demands in the way of food, &c., could not exist under the circumstances of the popular tales and superstitions "Arachnida" inquires about. The oldest fossil toads and frogs occur in *Tertiary* rocks. If, therefore, a live toad hopped, as has been alleged, out of a *Cretaceous* or *Devonian* rock, such a fact would amount to the declaration that the live toad could be ages and ages older than its fossil relatives, which declaration is, of course, the height of absurdity. There is no doubt that a frog or toad has an elastic constitution. It is cold-blooded; it can live under water for months; it can live for months after excision of its lungs, because the skin takes on the functions of lungs in such a case; and these animals (as in Dean Buckland's experiments) can live without food for a year or two, but, like all other animals (and plants), die *starved and meagre*, sooner or later. If "Arachnida" will only take the trouble to inquire into the *evidence* on which such stories as those he mentions are founded, he will find not one single *proved* or *provable* fact which will warrant any belief in the utterly impossible existence of toads or frogs in rocks. I may refer him for a fuller account of such cases to the essay on "Some Facts and Fictions of Zoology" in my "Leisure Time Studies" (Chatto & Windus). As a naturalist, I stake my reputation on the correctness of the views stated above, and also repeated in my book.

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EASY LESSONS IN BLOWPIPE CHEMISTRY.

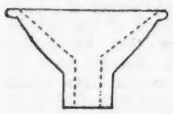
By Lieutenant-Colonel W. A. Ross, late R.A.

Introduction.

I FEEL some difficulty in expressing the satisfaction experienced by myself, in common, no doubt, with thousands of others, at seeing what has been for so long the great moral and intellectual *desideratum* of London—a *cheap* and simply-written, but at the same time thoroughly scientific magazine—so well supplied as it promises to be by Knowledge; and I beg, therefore, to offer you my humble efforts towards the accomplishment of this praiseworthy object. First, if I have your permission, in the simple exposition of a scientific subject which has attracted my most ardent attention, and extracted every leisure moment for its study, during the last twenty-two years of my life; secondly, by getting all the working people I can in my neighbourhood to spend their spare twopences weekly in the acquirement of Knowledge in the way of literature, instead of (as most do at present) in the purchase of trashy penny and halfpenny novels, boiled-down Scott, police *réchauffées*, &c., &c.

Lesson I.—How to Make a Blowpipe and Use It (Chemically).

Many English working young men already know how to use a common mouth-blowpipe in soldering and goldsmithing, &c., so that I shall have less difficulty in explaining how very little more trouble or pains are required in using it chemically. And first, as to making a blowpipe. We all know that a common conical, or "Black's blowpipe" (invented by the celebrated Dr. Black, of Edinburgh), made of tinned iron, with its brass nozzle, can be purchased for 9d.; but I would strongly advise every investor in such nine pennyworth, to add to his blowpipe a *trumpet-mouthpiece* (invented by the German chemist, Plattner), which any wood-turner will make of some hard-seasoned wood for 2d. more. If, however, my young friend is determined to make a blowpipe for himself, I will now show him how to make a far more powerful instrument than the above, and one, also, which does not require *any* art in blowing. A child can use it without being taught; indeed, my little boy (age 7 years) has already done so.



First, he must procure about a foot, in 2 in. pieces, of brass tubing, half of which should be 7-10 in. in diameter, and the other half *a little* smaller, so as to draw in and out of the former like a telescope, and yet fit air-tight. He must now solder a round piece of brass into one end of the wider tube, so as to close that for ever. These tubes are called the "Body-tubes" of the blowpipe. He can then make *the jet* of a small brass tube, tapering to a point, this form *and size*. Two holes are now to be correctly bored through

the sides of the wider body-tube above mentioned, about ⅛ in. from its closed end, so that this jet shall pass into them, closing air-tightly the larger hole at *a* and the smaller at *b*, which places are exactly 7-10 in. apart in the figure. A smaller hole like that in the figure is to be bored through one side of the jet, between *a* and *b*.

A brass *nozzle*, this shape and size, having a bore the width of a good stout sewing-needle, is to be fitted (but on no account screwed) on the tapering end of the jet. A child's indiarubber air-balloon, of the smallest size procurable, is tied on the thick end of the jet, with waxed silk-thread, and this part of my young friend's blowpipe is complete. To complete the whole, all he has now to do is to procure a wooden trumpet-mouthpiece to fit air-tightly into one end of the smaller of the body-tubes above described; but before fitting it, he must fasten over the inner end of its bore a strip of oiled-silk cloth, rather loosely, so that air blown upon the silk from the outside shall pass into the tube, but cannot pass *back again* into the mouth. This silk, in fact, forms a simple *valve*.

The young "pyrologist," or blowpipe-chemist, now possesses a blowpipe which, with proper "re-agents,"—cheap chemical substances with which to heat and treat his minerals, &c.—will enable him, after he has been through a course of these lessons, to "analyse"—that is, take to (chemical) pieces, so as to show what they are composed of—the "rocks" of the geologist or the "stones" of the mineralogist, belonging to a whole continent, if necessary (and if a decent time is allowed him for the purpose).

The Editor will explain far better than I can, that everyone's eyes see objects (small print, for instance) best at a certain distance. This point is called their "focus," and this distance their "focal distance." Most people's eyes have not only a different focal distance, but the focal distance of the same pair of eyes is apt (unhappily!) to alter through age; also in consequence of sickness or accident. The telescopic form of this blowpipe (which was invented by me about a year ago) enables each operator to draw it out or shut it up to the length suitable to the focus of his eyes. In packing, he draws out the jet, and, folding its air-bag as closely as possible, slips the whole into the smaller body-tube; that, with its contents, is passed into the wider body-tube, and the whole shut up, telescope fashion, forming a single short tube which can be carried in the waistcoat pocket.

In using the ordinary blowpipe, a little art is at first necessary, which generally requires from one to three days' practice, according to the capability of the learner. The best way to acquire the requisite proficiency I believe to be for the student to sit down with a lighted candle, having the wick bent to one side, on a table before him; to take his blowpipe in the right hand, not gingerly between three fingers, as some writers recommend, but firmly grasped in the fist; to apply the mouth-piece to his mouth with his cheeks puffed out, and elbow on table; to thus try to produce from the candle-flame, a "pyrocone," which is simply a cone of *blue* flame, breathing through his nose all the time. He will find this quite easy, but after a time, the air, or rather breath, in his cheeks will begin to fail, when all he has to do is to pronounce the word *cow* without opening his mouth. The effort to do this will replenish his cheeks with air.

HINTS TO LOCAL METEOROLOGICAL OBSERVERS.

(Reprinted, with the Author's permission, from *Science Gossip*.)

I. Hailstorms.

AS hailstorms are essentially local phenomena, it is to local observers that we must look for any addition to our knowledge of them.

A reference to any Encyclopædia article will show the sadly confused state of our information on the subject. Flat contradictions will be found on every page. It will be seen that while some authorities assert that hailstorms occur most frequently in summer, others maintain that winter is pre-eminently the hail season. There must be an error somewhere. As a matter of fact, false statements have been so persistently reiterated, that by mere repetition they have come to be regarded as truths.

Let me allude to one fertile source of error. I have referred to it before.* It is the confusion of hail proper, or French *grêle*, and winter hail, or *grésil*—two entirely different phenomena. *Grésil* is the small round powdery snow which often falls towards the end of a snowstorm and in the early part of a very frosty night. I suspect that Dalton and other observers did not distinguish between the two kinds of hail, and spoiled their results in consequence.

The following points are of primary importance, and should not be overlooked in taking down an account of a hailstorm:—

1. *Period.*—The date and hour at which a hailstorm occurs, though apparently small matters, are of the utmost importance. I am inclined to think that while each country has distinct maximum and minimum periods of hail-fall, the distribution of the quantity over the year varies materially. For example, in this country, the maximum occurs in summer, and is very strongly marked; while in Germany, the maximum is in spring, and is not so decided. If the periods of maxima and minima all over the world could be determined, a comparison of them would doubtless throw much light upon the nature and cause of hailstones.

2. *Area.*—When a hailstorm is not purely local, it usually assumes the form of the tornado, and sweeps over the country in one, or sometimes two, narrow bands. In the former case, it is sufficient to ascertain the area covered by the storm, but in the latter, it is necessary to determine (*a*) the length, (*b*) breadth, (*c*) direction of motion, and (*d*) rate of progression of the storm-band. Such particulars can only be obtained by the co-operation of numerous local observers. Our organised observatories are quite incompetent for such work, being too sparsely distributed over the country.

3. *Physical features of the locality.*—The proximity of mountains seem to induce the fall of hail, while that of forests has the opposite effect. Progressive storms often diverge from their course on encountering a river or valley, and follow that of the depression. It is therefore useful to note if any of those physical features be in the neighbourhood of the storm, and if so, their effect upon it. It is also of importance, especially in the tropics, to determine the elevation of the country above the sea level. It is frequently asserted, on what authority I know not, that hail never falls in the

* *Nature*, vol. xxiv. pp. 187-90.

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tropical regions at a less elevation than 2,000 feet. It is worth while testing the statement.

4. *Temperature.*—The fluctuations of temperature during a hail-storm are often very remarkable, and should be carefully observed. A reading of the thermometer may be taken shortly before the storm begins and another directly on its cessation.

5. *Barometrical readings* should be taken, if possible.

6. *Wind.*—Its (*a*) direction near the earth's surface, (*b*) direction in the higher regions as indicated by the cloud motion, and (*c*) force, are important points. Some observers have noticed that the clouds move in various directions while a hailstorm is in progress. Kämtz actually went the length of attributing the formation of hail to the conflict of opposing winds; and Beccaria says, "While clouds are agitated with the most rapid motion, rain generally falls in the greatest plenty; and if the agitation be very great, it generally hails." Howard, in 1809, noticed the wind change from E. to S., then to W., back again to E., and finally to W., during a hail-storm.

7. *Rain.*—Rain sometimes falls before hail, sometimes after it. The area of a hailstorm is generally fringed with rain, and in the case of a moving storm, rain falls along both edges of the track. Rain before hail is somewhat rare, and its occurrence should be carefully noted.

8. *Clouds.*—Hail clouds are invariably cumulus. Volta and other theorists have assumed that there are always two strata of clouds at different elevations. Arago pointed out that they are generally of an ashen hue. Their aspect, apparent thickness, and height above the earth may be noted.

9. *Electrical phenomena.*—It is frequently stated that thunder and lightning always accompany the fall of hail, but such is by no means the case. When there is lightning, it is important to observe the relation between the discharges and the fall of the hail—whether the lightning precedes the hail, or *vice versâ*. If possible, the electricity of the air before and after the storm should be ascertained by means of an electrometer.

10. *Duration.*—The duration of the storm at one spot may be noted.

11. *Preliminary sound.*—Kahn, Tassier, Peltier, and others affirm that they have heard a peculiar rumbling or pattering sound in the air immediately before the descent of hail. This cannot be a common phenomenon, or it would have been more generally remarked.

12. *Structure and size of the hailstones.*—Observations of the structure of hailstones are seldom of any use, as the necessary precautions are generally neglected in conducting the examination. The ice of which they are composed undergoes a rapid change when exposed to a high temperature, so they ought to be collected immediately on descent. Further, as collision with the ground is liable to cause alteration of shape, if not entire fracture, it is well to catch the hailstones destined for examination upon a piece of flannel, which not only preserves the stones in their entirety, but, being a bad conductor of heat, keeps them from dissolving rapidly. The scrutiny may then be conducted in a cool room. Size should be determined by accurate measurements. Such vague terms as the "size of peas," or the "size of eggs," or "like large nuts," are useless for scientific purposes.

The above points may be supplemented by any others that the experience of observers may suggest or that peculiarities in individual storms may require.

J. A. Westwood Oliver.

Athenæum, Glasgow.

ANECDOTES OF DOGS.

I SEND you some anecdotes of dogs, which, I think, show most strongly that they not only have powers of reason of the same nature as ourselves, but that they share with us distinctly some of our virtues. To me, dogs have always appeared to be by far the nearest animals to man in their intelligence, and in their evident sympathy with their masters, and they show this latter by a power of expression in their faces which no other animals have a trace of. Nothing can be more distinct than the smile of pleasure which lights up a dog's face at the approach of a kind master, and that such an animal should ever become the victim of the tortures of the physiologist is to me inexpressibly horrible.

The following facts, though not actually occurring under my own eyes, were related to me by friends who witnessed them, and I am as certain of the truth of the stories as if I had been myself present:—

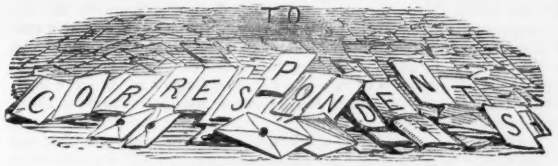
A lady residing in a house not a hundred yards from mine has a pug dog, also a cat of which the dog was always very jealous, chasing it about whenever it saw it. Not many weeks since the pug astonished its mistress by coming up to her, sitting up and begging, then barking and running a little distance from her, till it appeared evident that it wished to persuade her to come with it. It continued to beg and to run on in the same manner till it led her out into the garden, to the foot of an apple-tree, against which the dog raised itself on its hind legs and barked vehemently. On looking up the lady saw the cat with a trap on its foot, evidently in great pain. She got it down and relieved it of the trap, the dog showing the greatest joy, and on the cat being placed on the ground, the dog, who before had never done anything but hunt and worry it, licked it all over and over till it was quite wet, and ever since they have been the best of friends.

A dog had a kennel in the yard of a house which was overlooked from one of the windows. A lady, my informant, saw this dog hiding some of its dinner in a corner behind the kennel, and this performance, she noticed, was repeated for a day or two. On the third day the dog was missing some little time from the yard, but before long it was seen to return, followed by a small, half-starved canine friend, which it took up to the store of hidden food, and stood by, wagging its tail with evident pleasure, while the strange dog consumed it. Now, if these two stories do not show the virtue of charity in a dog, I don't know what can be required to prove it.

The following is of a different nature. In Ceylon the large Lambur deer is hunted by dogs, the huntsmen going on foot. The deer generally comes to bay in a stream among the hills, and the huntsmen, guided by the sound of the dogs, make their way to the spot as quickly as they can with knife or spear to end the combat. Sometimes, however, as the distance, or the form of the hills, prevent the sound of the dogs at bay from being heard, the huntsmen do not arrive, and the deer, if strong, may escape, or is, perhaps, killed by the dogs. On one of these occasions, the owner of a pack of hounds, who related this to me, lost all sound of the hounds, and came back home to breakfast. After this he sallied forth again, thinking he would go to a distant part of the jungle, where he imagined the pack might have taken the deer. Before he had gone a mile or two he met two of his pack by themselves, coming straight for home. They no sooner saw their master than they expressed the greatest delight, and at once turned round, went before him, and led him straight through several miles of jungle, to where he found all the rest of the pack, with a large buck Lambur at bay. They were quietly waiting round it, preventing its escape, and, on seeing their master, the jungle at once resounded with their voices, as they went in with renewed energy at their quarry, till the knife of their master put an end to the battle. Now, it was perfectly clear to my friend that these dogs had agreed among themselves that two of their number should go home and fetch their master, while the rest kept the deer at bay.—B. M., F.R.C.S.

Knowledge.—Although we offer our readers more in the way of original matter (apart from correspondence, which is not to be estimated by mere bulk) than any other journal of similar price and character, we wish to do better still. We hope so to extend the circulation of Knowledge that we may be justified in enlarging each number, in giving more illustrations, and in extending the number of our original contributors. *To attain this end we need the co-operation of our readers.* Those among them who approve our scope and plan can do more to improve Knowledge than either editor or publishers. If every reader were to obtain but one new subscriber, not only would our circulation be doubled, but our power to improve the matter placed before our readers would be increased in like proportion. If our readers will remember this, they will follow the best course for making Knowledge what we wish and hope that it may before long become.

Wordy Science.—It has always been characteristic of a science, real or so-called, which for any reason was not advancing, that its professors have endeavoured to give it an unreal importance by heaping over its facts a mass of incomprehensible verbiage; and the traditions of such a time have survived, in many instances, long after the branch of knowledge concerned has begun to share, or has shared even in the most marked manner, in the general advance of the human mind. Botany, chemistry, the smaller ramifications of natural history, are all still concealed beneath technicalities which are neither English, nor Greek, nor Latin, nor hybrid of any declared description, which convey no meaning, are not generally intelligible to the learned, and are not intelligible, indeed, to any, save those who have wasted precious time in learning them by heart from text books. Such technicalities are unmixed evils, because they do nothing in reality to facilitate the acquirement of knowledge, and they hinder many from even attempting to acquire it.—*Times.*



Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

DEMANDS ON OUR SPACE.—CURIOUS PUZZLES.—GRAMMAR.—CENTRIFUGAL FORCE, &c.—A PARABLE FOR PARADOXERS.—THE EARTH'S PROCESSIONAL REELING.—ILLUSIONS.—BRAIN AND SKULL.—MARRIAGE AND THE DEATH-RATE.

[121]—A correspondent remarks, and with some justice, on the space awarded to correspondence, queries, and replies to queries in No. 6. It is natural that in the first number of a magazine admitting correspondence at all, the space devoted to such matter should be much less than in later numbers. As correspondence (including notes and queries) was mentioned in the prospectus, I cannot consider that the increase of this section in Nos. 2, 3, &c., was inconsistent with our promises. But, while noting this, and that, indeed, number 6 was enlarged to oblige correspondents, I must admit that it would not do to allow correspondence often to occupy so much space. As letters come in more and more freely each week, and as we have not room for more than a third of them, it is clear that two-thirds must be omitted. Those that are most original and interesting will naturally have the best chance of being retained, and among these the concise will be preferred.

Some letters will be given in abstract, with such reply as may seem necessary, in a weekly communication from "the Editor."

"H. A. S." notes that there is an exhaustive solution of the problem of the Fifteen School-girls in the "Ladies' and Gentlemen's Diary" for 1862. Magic squares are treated fully in Hutton's "Mathematical Recreations." Nevertheless, I think the three letters on these two subjects published in this or following numbers will interest many who would not care to look up the above-named writer. They may suffice, however, for matters which are rather curious than scientific.

"L. F." remarks (*see* "A. T. C.'s" letter, 105, p. 117) that "would the editor" is more deferential than "will the editor," "might I" less manly than "may I," "I cannot but think," "I can but think," and "I can only think" equivalent expressions, He notes justly that such a sentence as "the editor requests that letters may be addressed," is incorrect. "May" should be omitted. The same with "he requests that all communications (should) be addressed," &c., "should" should be left out; or, better, "he requests that" should be left out. We have under our correspondence heading "he requests that all communications should be as short as possible." Elsewhere in our remarks, be it noticed, we adopt the correct form. Perhaps we meant to say editorially, "we request that the principle be adopted 'all communications should be as short as possible.'" We take "L. F.'s" hint, however, and put this principle down definitely as one which we are obliged to insist upon.

Mr. Newton Crosland is, I fear, offended because I have said that he misapprehends the principles on which the accepted theories which he opposes are based. I am very sorry. I have no wish to offend him. I have simply stated the facts as I see them. He describes the centrifugal force in terms entirely inconsistent with the real nature of this so-called force. (He is quite right in saying that Newton spoke of the centrifugal force; so also Newton spoke of the force of inertia. Newton, however, carefully distinguished these from "impressed forces." Modern science, to avoid precisely such misconceptions as Mr. Crosland's, ceases to use the term force except in the case of what Newton called an impressed force. Joyce, Milman, and Ferguson are not authorities in such a matter, not one of them having had any mastery of the higher mathematics.) Again: Mr. Crosland, in dealing with the Darwinian theory, uses words only applicable to the Lamarckian hypothesis. A student of science knows the Newtonian theory and the Darwinian theory, attacked by him, to be unlike what he supposes; and, therefore, is justified in saying to him that very likely these theories, so misunderstood, appear egregiously absurd. And there the matter should end. I am not interested, and I am sure readers of KNOWLEDGE would not be, in defending the straw-giants which Mr. Crosland attacks after having constructed them. But there is nothing which need offend him in the remark that he misunderstands the teachings of Newton or of Darwin. Many misunderstand both. Of the former, at any rate, which belongs to my own special department of study, I may say that few understand them. Many who understand the Darwinian theory, reject it, or regard it as so far not perfectly proven. But no one who understands the Newtonian theory questions its truth. Any one who, understanding it, yet objects to the former theory, will find his reasoning admitted here. Any one who does not understand any particular point about either theory can ask here for explanation. But all our space would be wasted if we allowed correspondents—first, to set up burlesques of scientific theories; secondly, to overthrow these shams; and, thirdly, in the triumph of an imagined victory, to invent vague theories of their own, in establishing which they assign to imaginary forces inconceivable effects.

A chemist in New Zealand once asked me, as a believer in the Newtonian theory, how I explained a balloon's ascending. Before I replied, he went on to say that, to his mind, the ascent of a balloon proved that the earth exerts a repulsive as well as an attractive influence. There was a pair of scales on the counter, in one of which I put a half-ounce weight, and as the other scale went up, I asked if its motion was due to terrestrial repulsion. "The cases are different," he said. "The cases are similar," I answered; "the light scale ascends because the heavier one has a greater tendency to descend, and cannot descend unless the light one rises; the lighter gas in the balloon ascends (taking its silk coat along with it), because the heavier air around it has a greater tendency to descend, and cannot descend unless the lighter gas rises. The heavier scale pulls up the lighter, which would else tend downwards; the heavier air thrusts up the lighter gas, which would otherwise descend as certainly as a globe of lead would." Whether the chemist understood or accepted my explanation, I do not know; but this I know, that if he had based on his misapprehension of the effects due to gravity a theory of polarity and magnetic repulsion, I should not have felt bound to discuss his views with him. In a similar way, acting according to my lights, and with the object of occupying the columns of KNOWLEDGE so as best to serve the wants of readers, I must decline to assign more than a very modest allowance of space here to paradoxical theories.

"Sirius" asks for an explanation of the "Precession of the Equinoxes." As this is a subject of general interest, I do not put this letter among the other queries, but reserve it for an answer in an early number. I note here that the change is *not* such as to cause the northern pole to be inclined from the sun in June 13,000 years or so hence. Nor is the motion of the poles such that 6,500 years hence, or at any time, the axis of the earth will be at right angles to the ecliptic. The inclination of the axis varies only within a very narrow range, and the seasons remain almost unchanged in each hemisphere, throughout the long precessional period of nearly 25,900 years.

J. E. Okill remarks that the "nest of illusions," p. 71, is illusive to him, as respects the apparent superposition of the space between the two heavy circles, only when he expects to find an illusive effect. If he looks along the lines as a carpenter looks along a piece of wood, they seem straight, and the circles seem oval. (This, of course.) He describes as an illusion what is in reality an effect of diffraction. Closing one eye, look at the edge of a window with the other, covering this eye gradually with the hand till only a fine slit is seen, when the colours of the spectrum are displayed. He remarks on letter 114, p. 121 (Brain and Brain Case), that the two doctors are in agreement. Naturally, the skull takes first a form determined by the growth of the brain, while later the brain may shrink away from its bony enclosure. Also, "may it not be that the son of a man noted for skill in some art or science may inherit the shape of his skull," but, for want of study, the brain

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may not fill the corresponding expansions of the brain-case. He replies to Query 27, that the greater number of deaths among unmarried men may be due to evil living, which the married, having home comforts, avoid; also, in part, to neglected ailments. The question really of interest is, however, whether the married life is in itself a prophylactic. We want better statistics than we have. The greater death-rate among widowers seems to support the views of Bertillon and Stark; but, manifestly, a cause may act in their case which affects the statistics apart from any influence the married life may have. Richard A. Proctor.

JUPITER—DENNING'S COMET.

[122]—I trust that some of your readers will obtain observations of a remarkably bright spot situated slightly S. of the equator of Jupiter, and on the N. edge of the great S. belt. I have observed 61 transits of this spot since October, 1880. The following are the approximate times when it comes to the central meridian of Jupiter:—

Dec.	H.	M.
17	10	40
18	6	20
19	11	51
20	7	31
21	13	1
22	8	41
23	4	22
24	9	52
25	5	32
26	11	2
27	6	43
28	12	13

Its period of rotation is 9h. 50m. 6s.6., or 5½ minutes less than that of the red spot, which is 9h. 55m. 35s. It is in the same longitude as the middle of the red spot on December 25, and these conjunctions occur at intervals of 44½ days.

The last time I saw the bright spot it was very plain. This was on the night of December 7, when it was noted exactly on the central meridian, at 4h. 41m. and 14h. 33m. These double transits are not unfrequently visible now that Jupiter is above the horizon during the greater part of the night.

The new comet (of 1881) was observed here on November 25, at 10h., and again at midnight. It was pretty bright and large, with ill-defined and apparently flashing edges. There were many faint stars in the background, so that I could not be certain as to the character of the nucleus.—Yours, &c., W. F. Denning.

Ashley-down, Bristol, *Dec.* 8, 1881.

THE TRUE SPIRIT OF SCIENCE.—WATER SPHERULES.

[123]—Let me venture to express a hope that the concluding portion of the lines from which you have taken your motto may, as I doubt not they will, find expression in your magazine,—

"Let Knowledge grow from more to more,
And more of *reverence* in us dwell."

The spirit of reverence and of humility, the spirit of Newton, Herschel, and Faraday, seems gradually vanishing in many high scientific circles, and a spirit of self-sufficiency and of arrogance growing in its place. It is sad to see how frequently the student of nature of to-day, in the consciousness of his acquisitions, referred to yesterday, forgets the absurd littleness of his knowledge referred to the sum of all things.

But I must not stay to pursue this thought, as my object in writing to you was to refer to a question asked in the copy of Knowledge sent to me, No. 3, p. 60, where a correspondent asks what supports or causes the spherules of water which are often seen rolling on the surface of water. The explanation hypothetically given in your columns is, as you suspect, wholly untenable. The probable cause is a *difference of temperature* between the drop and the surface on which it rests.

Apropos of this, the accompanying short paper may, perhaps, interest some of your readers. It was read at a meeting of the Royal Dublin Society on Dec. 15, 1877, but, so far as I know, has not been published out of the proceedings of that Society.—Yours, &c., W. F. Barrett.

[The paper shall appear shortly.—Ed.]

THE WEATHER FORECASTS.

[124]—Taking great interest in the daily weather forecasts as efforts made by the director of the Meteorological Office and his staff to predict the weather in advance, I have for about two years past been in the habit of noting briefly the characteristics of each day, and comparing them at night with the predictions as published in the *Daily News*. I note occasional discrepancies—not all the newspapers printing them alike. The variations in phraseology are, however, generally unimportant, consisting in the elision of the word "very," or making trifling variations in the direction of the wind. For the period of the last six months my notes have been fuller than formerly, so that I will confine my observations to these more particularly.

As the main conclusion, I am bound to say that they are about as often wrong as right in the metropolitan area, to which my attention has been almost exclusively confined. There is great difficulty in drawing a sharp line between "successes" and "failures," for often the temperature, force, and direction of wind have proved right; but the forecast as to rain or fair weather has frequently been altogether unsuccessful, so that it is not easy to state the percentage of successes.

I have noted particularly eight occasions of excessive rain or thunder-storms happening within the months of last summer, not one of which had been predicted in our daily forecasts, although, strange to say, clear warning had sometimes been sent from America, as was noticed by a writer in the *Times* of July 9 last, signing himself "Observer." The dates I now refer to are—May 28, when we had a thunderstorm and torrents of rain in the afternoon, certainly not predicted in the forecast, "cloudy, but *fair* and warm;" June 5, when we had more than half an inch of rain—not predicted; June 25, eight hours' rain, from 10 a.m. till 6 o'clock in London—not predicted, but coming after a week of gloriously bright weather. Then came the heavy thunderstorm of July 5–6, to which I have already referred, bringing down the temperature from about 92° F. to 70° F. in one day; a disturbance not foretold by our Meteorological Office, although a warning from the *New York Herald* had already appeared in all the newspapers. July 28, when the prediction was "*fine*, but cool in the morning." Right as regards temperature, but wrong as failing to give notice of the torrents of rain which fell before midnight. August 8, the weather predicted was "cloudy or dull at first, then clearing again, and *fine and warm*," instead of which the weather in London was fine until 4 p.m., followed by six hours' rain, which brought down the temperature so low, that at noon next day the thermometer only registered 55° F. August 12, the prediction "cool, changeable, showery," did not prepare the sportsman for continuous rain from morning till night (sixteen hours, at least, without ceasing) and not well described by "changeable" or "showery." The direction of the wind was wrongly predicted, proving East and North-east, instead of "Westerly to North-westerly," and almost calm in London, instead of "strong and squally." Passing over August 16, when we had a fine morning, succeeded by a wet evening (the reverse of the prediction), and August 18, a fine calm day, not "squally" or "showery," as predicted, we come to August 25, raining all day all over England (see the column of reports in the *Daily News* of the following day), and certainly not fulfilling the predictions "fine generally," or "greatly improved weather" promised for the English districts Nos. 2 to 5 inclusive, but, on the contrary, everywhere spoiling the harvest prospects, and remaining bad weather for the rest of the month.

Later instances of "bad shots" are in my note-book, but for the purpose of discussion the above may suffice, and with every desire to keep within the bounds of fair criticism, I wish only to point attention to the facts, in the hope that the causes of failure may be discovered and eventually remedied. Mr. R. H. Scott, in his article on "Forecasting the Weather," published in *Good Words* of July and August, admits his "inability to form any estimate of the amount of rain which is likely to fall on any day," although continually professing to take account of rain in his daily forecasts. The whole subject gained importance from the question asked last August, in the House of Commons, by Mr. Earp, M.P., as to the facilities for the promulgation of those weather-forecasts which the Postmaster-General might be inclined to give? and the seaports and agricultural towns have now the chance of getting information on very easy terms. It remains to be seen whether the local authorities rightly estimate the efforts of the Meteorological Office to provide this much-needed information. The end can only be attained after many failures, for, as Professor Huxley says, "all true science begins with empiricism." In what directions, then, are we to look for improvement in the character of the weather-forecasts? And what new means can be devised to give them greater accuracy? These are questions which appear to be well worth discussion in the pages of Knowledge.—I am, yours, &c.,

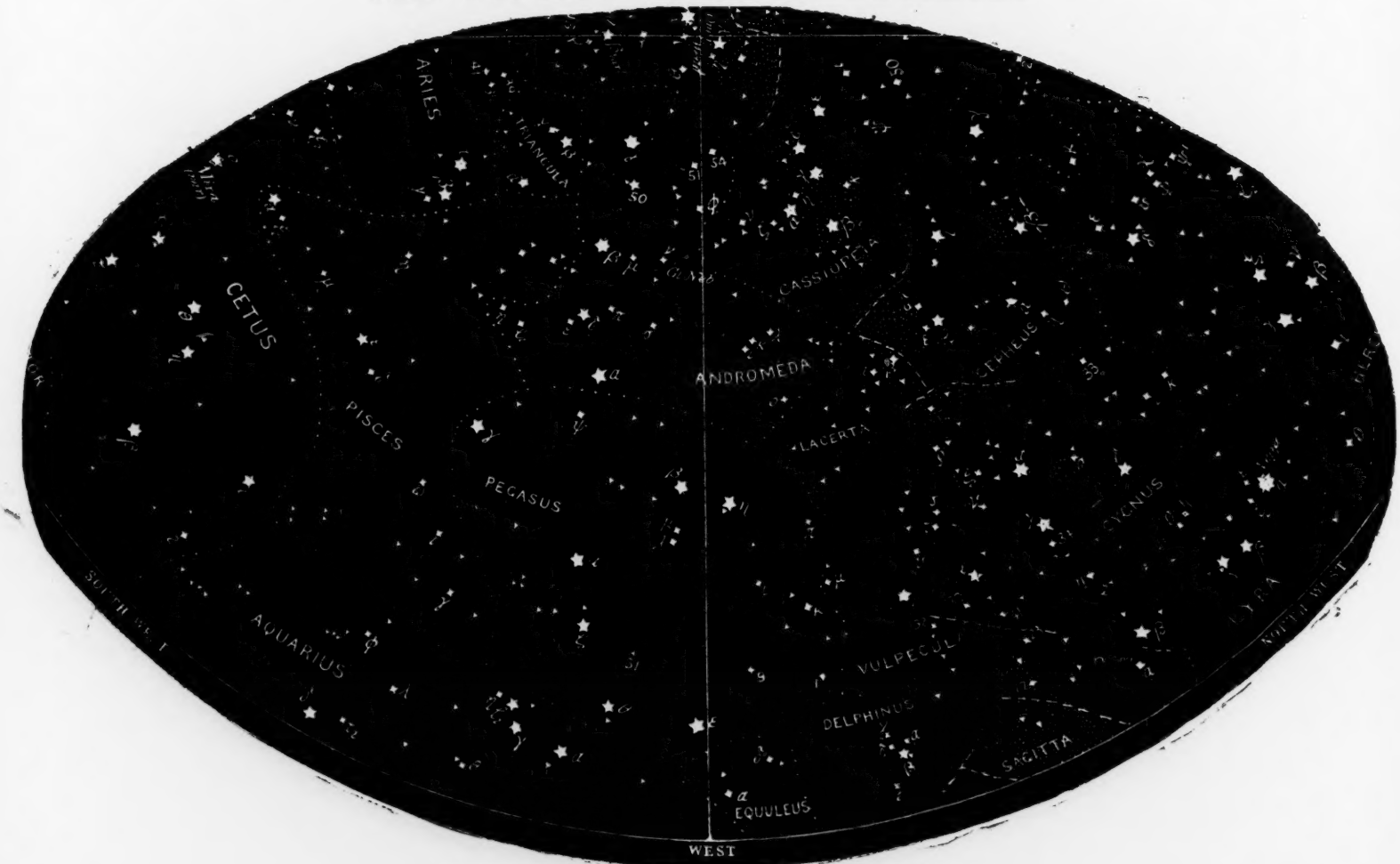
John Spiller.

WHY RATS GNAW THROUGH A WATER-PIPE.

[125].—The moisture of the atmosphere is condensed on the outside of the pipe during a change of temperature (the cold water in the pipe and the moist warm atmosphere), which induces

[*Continued on page* 143.

THE WESTERN SKIES IN DECEMBER.



This Map shows the position of the stars in the Western Skies: —

On November 30, at 10½ o'clock. On December 4, at 10¼ o'clock.	On December 8, at 10 o'clock. On December 12, at 9¼ o'clock.	On December 16, at 9½ o'clock. On December 19, at 9¼ o'clock.	On December 23, at 9 o'clock. On December 27, at 8¾ o'clock.	On December 30, at 8½ o'clock. On January 3, at 8¼ o'clock.

The stars in the western skies are passing downwards towards the horizon, moving aslant from left to right; but those shown on the right, or northern side of the map, will not set, but pass over towards the north, there to pass below the pole.

*** Stars of the first magnitude are shown with eight points, those of the second with six, of the third with five, of the fourth with four, of the fifth with three.

Continued from page 140.

them to drink, and as the supply is only small, they naturally gnaw through the pipe. This, in my opinion, is the more correct view than to imagine they reason from hearing the fluid pass through the tube. W. MARSHALL, F.B.S.E.

ELEMENTARY ELECTRICITY.—SCIENTIFIC MEETINGS.—MYSTERY OF "PSYCHO."

[126]—Having read letter (No. 73), signed "Daniel Jones," I should like to point out that it seems to me very undesirable that you should use your valuable columns for any such papers, which can be read in any good text-book on electricity.

May I suggest that you should find room in your columns for a notice of times and days of the meetings of scientific societies during the ensuing week, with the titles of the papers to be read.

I should also like to ask through your columns whether a solution of the mechanism employed in the automaton "Psycho" has ever been given?—Yours, &c. H. W. B.

[We quite agree with our correspondent on the first point. As to the second, we believe that only a small proportion of our readers care to know the days on which the various scientific societies meet; and that all who do care, must be in the way of learning those dates elsewhere. It seems, therefore, better for the greater number—by far the greater number—to save for other purposes the space which such notices as "H. W. B." suggests would occupy. —ED.]

NAMES OF STARS.

[127]—I am making a collection of the ancient names of stars. In this I have been much assisted by the list given in your larger Atlas. I should be very much obliged to you if you could point out any books in which I could find more of these names, as well as any traditions about them. A friend of mine, who is an Arabic scholar, has promised to translate those which are not too much corrupted. It seems a pity that these names should stand a chance of being forgotten, as much of the popular interest in stellar astronomy dies out when stars are only designated by a letter in a shapeless constellation. Apologising for thus troubling you, Yours, &c., J. H. THOMSON.

R. A. Mess, Shoeburyness, *Nov.* 28, 1881.

[There is much that will interest Mr. Thomson in "Smyth's Bedford Cycle," the old edition. From the new one we understand the matter in question has been removed.—ED.]

DONATI'S COMETS.

[128]—In Figures I. and II. on page 49 we have two pictures of Donati's comet, as also, judging from the stars given in the diagrams, the distance and direction travelled by the same. Now, if the stars depicted on the plate have relative places in the firmament, the comet must have been travelling in a side direction, and consequently the tail must not have been pointing from the sun.

A YOUNG ASTRONOMER.

[The picture is right. There is no such difficulty as "Y. A." supposes. The comet's path is not on a line towards the sun.—ED.]

PASTEUR'S PLATES OF GERMS, &c.

[129]—Would Dr. A. Wilson, if possible, kindly give us Pasteur's plates in connection with the subject. He may intend to do so; if so, my suggestion is premature. I am sure they would be interesting. A friend, a few years ago, lent me Pasteur's plates on the germs generated in brewing, and many a pleasant hour have I spent in trying to obtain the different germs by experiments on beer, yeast, &c.

I have read a good many of the scientific papers of the day, but have never been so interested in any of them as I have in KNOWLEDGE, which, I feel sure, has only to be known to be a complete success and boon to the public.—Believe me yours, &c.

F. C. S.

COLOURS IN ANIMALS AND PLANTS.—DARWIN ON WORMS.—REASONING ANIMALS.

[130]—The tendency of men of science to run investigation to extreme tenuity is become a danger. Knowledge whittled to a point is knowledge reduced to nothing. Casual inquirers recoil from infinitesimals and the overwrought.

Dr. Wilson, in KNOWLEDGE, No. 2, labours to show that the colours of wild birds and beasts are designed by Nature to obscure the animals from their enemies, and, when predatory, from the eyes of their prey. It might be affirmed that the colours of flowers are designed to put the flowers conspicuously before the eyes of men and insects. Nature is not given to special amenities and providences. The dark colours of the sole and the flounder are no protection. When in repose, they are concealed by a covering of sand.

Mr. Darwin's theory of worm action on the soil is, to my mind, an elaboration of exceptional minutiæ to huge paradox.

The paper on Intelligence in Animals is in another category. Anyone capable of accurate observation, and daily among them, knows that animals do reason. I could name many instances, but one shall suffice. A duck came opposite our day-room window, which looks into the orchard. Mrs. D. noticed her uneasiness and persistent waiting, and concluded that it indicated hunger. She took madam duck some corn. The food was simply looked at, but the look was followed by movements that plainly meant "come with me." The request was accorded to, on supposition that the duck was shut out of her cote. But, no; the cote was open, and the duck urged "come on, come on." She led to a hole in the hedge that fenced the stackyard from the orchard; and there, on the other side, was a lame duck, that could not make passage of the hole, in a fever of despair. Mrs. D. went round and brought the poor bird into the yard. Meanwhile, the kindly-hearted duck made her way to where she knew her mistress would return, and the rest of the flock gathered with her. The meeting of the two ducks was a little drama. Between them there was a wonderful talk, soon joined in with by the whole flock; and they marched off with what was taken to be loud cheers.

Reason! Surely animals, furred and feathered, reason; some better than others, but all reason. The fox is a rapid and acute reasoner.

I have but limited intimacy with pigs. So far, however, as I am acquainted with them, nothing is so manifest to me as that they reason; and they are humorous and inclined to fun, when at liberty. A well-known sportsman had a pig for pointer. Dogs all reason, the high-bred sheep-dog in particular; and more, understand sentences of speech addressed to them directly, or in their hearing, and in the latter case, they will, if so minded, circumvent you on the knowledge they have gained.

Ducks and dogs have the human propensity to sulk with you, or with one another.

Memory, no observer will deny them. But is not an effort of memory reasoning; calling together ideas previously known, and linking them, absolutely or tentatively, in sequence, till the desired lost idea is regained?

Professor Jevons' logical abacus, in work, really represents the mode of an effort of memory, as well as a logical concatenation.—Yours faithfully, B. DONBAVAND.

Picton, Chester.

ATMOSPHERIC ABSORPTION AND TERRESTRIAL RADIATION OF HEAT.

[131]—In his "Heat a mode of motion," (1880) page 317, Prof. Tyndall says:—"I never on any occasion suffered so much from solar heat as in descending from the 'corridor' to the grand plateau of Mont Blanc on Aug. 13, 1857. Though Mr. Hirst and myself were at the time hip-deep in snow, the sun blazed against us with unendurable power." Mr. W. Mattieu William's letter 68, page 96, and B.M., F.R.C.S., No. 36, page 77, bear similar testimony. I think Prof. Tyndall explains the whole matter (page 317). "The beams of the sun penetrate glass without sensibly heating it; the reason is, that having passed through our atmosphere, the heat has been in a great measure deprived of those constituents liable to be absorbed by glass." Prof. Tyndall refers to the invisible (the hottest) rays, see page 316. It seems to me too much stress is placed on the atmospheric absorption of terrestrial radiation. From the foregoing references a large proportion of the sun's invisible rays are intercepted before they reach the earth; and then all those which do impinge on the earth raise its temperature, and by "convection," more heat is transferred to higher altitudes of the air, so that only a comparatively small proportion of the heat is radiated from the earth compared with what enters the atmosphere as solar radiation. J. A. L. R.

[A large portion of the invisible solar rays remain (compare figs. 112 and 113 in Tyndall's book) after passing through the air, and to these, constituting at least twice the visible rays, the reasoning of Mr. Dyer (in letter 20, p. 56) applies.—ED.]

"KNOWLEDGE" AND THE SCIENTIFIC SOCIETIES.

[132]—I see that some of your readers are asking for Reports of the Proceedings of the Learned Societies, and that you have replied very practically by showing them a sample of what the most meagre outline would amount to.

In confirmation of your decision, I may state my own practical experience. When my genial friend, the late George Dawson, started the *Birmingham Morning News*, I undertook the functions of

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"Our London Scientific and Educational Correspondent," whose work was to supply a weekly column or two, by means of which the readers of the paper should be kept acquainted with the general progress of science and scientific education. My original programme included an account of the proceedings of the Scientific Societies, but I met with a rebuff at the outset. The secretary of the Royal Society informed me that I should not be admitted to any of their meetings if I reported any portion of the discussions or conversations following the reading of their papers, and that they would rather not have any newspaper anticipations of the official publication of the paper themselves. The titles of the papers were at my service. I began by heading each of my communications with a list of the papers read at the principal London Societies, and supplementing this by a short notice of some of those likely to be popularly interesting; but alas! how few were they—not 5 per cent. of the whole. This was in January, 1871. By the middle of May, I gave up even the titles of the papers, finding them about as readable as the Post-office Directory, and practically worthless as a record of scientific progress, seeing that London is not all the world. If I had added the titles of papers read at foreign societies, my weekly column would have been filled with these alone.

To give your readers an idea of what would become of Knowledge if you gave abstracts of current scientific papers, I have counted the number included in one month's *Journal of the Chemical Society*, wherein abstracts of papers read outside of the Society are given:—

General and Physical Chemistry	26	papers
Inorganic Chemistry	25	,,
Mineralogical Chemistry	74	,,
Organic Chemistry	113	,,
Physiological Chemistry	7	,,
Chemistry of Vegetable Physiology and Agriculture	10	,,
Analytical Chemistry	23	,,
Technical Chemistry	23	,,
Making a sum total of	371	papers

in one month (June last) on one branch of science only. A staff of thirty experts, Fellows of the Society, are engaged in making these abstracts. The annual volume of abstracts (all very short) averages above 1,000 pages, besides the transactions, which usually run to about 800 pages.

To the professional chemist these are invaluable; to the general public they are mere waste paper.

W. Mattieu Williams.

THE MISSING LINK.

[133]—This question seems to generate new ideas (and not always reasonable ones) as time passes. One correspondent, "Another Ignoramus," says: "Common ignorance (?) will continue to ask the unfortunate question." Why "unfortunate" is more than I can imagine. In my previous answer I tried to show that the phrase "missing link" was a misnomer. It implies that there was but *one* link connecting man in the past with lower life, and it thus implies an amount of knowledge which honest science declares it does not yet possess. Persons whose demands are all for the "missing link," generally ignore the overwhelming evidence of evolution as a fact of nature that Mr. Darwin, Mr. Wallace, and others present. Nobody calls anybody else an "ignoramus" except those bigoted minds who object either to receive evidence, or who are unable to appreciate and weigh evidence of evolution or any other topic. I certainly called no one an "ignoramus." On the contrary, I did my best to assist a comprehension of the question concerning man's relationship to his lower neighbours. Any one may object to the views of another, but objections should be stated in full. Querulous complaint assists no cause, least of all that of scientific truth.

"Mitchell" likewise writes on the "Missing Link" subject. Has "Mitchell" any knowledge whatever of geology, or of the history of fossils? I ask this, because if (as I suspect) he expects to find every animal and plant species that lived in the past preserved as fossils, he is simply quarrelling with Nature's flaws, and neither with evolution nor with me. I refer him to the "History of the Horse," as disclosed by Professor Marsh in America, for an excellent illustration of the transformation (or evolution) of one order, genus, and species into another. He will find the account in Huxley's "American Addresses." To answer his second inquiry, he should acquire a knowledge of geology. "The imperfection of the geological record," as treated by Darwin in the "Origin of Species," will give him a capital outline of the whole case, and an acquaintance with geology as a whole will show him that the probabilities of evolution are overwhelmingly supported by the conclusions drawn from fossil history. People must know there is no "royal road" to the truths of nature, either in the pages of Knowledge or anywhere else. My advice to "Mitchell" and to "Another Ignoramus" is, to acquire a general knowledge of biology and geology in a class-room, or by a course of wide reading. Otherwise they will always be encountering difficulties, or, perchance, complaining of the lack of light in others.

Andrew Wilson.

DIFFICULTY OF OBTAINING "KNOWLEDGE."

[134]—Can you tell me why there is so much difficulty in procuring Knowledge? I never had so much trouble in procuring any periodical or paper before. So far, I have never been able to procure any of the numbers until perhaps six, seven, and eight days after date of publication, although it is regularly ordered (every day) by my bookseller here, who has a London parcel daily. Every time I call at the bookseller's I meet with the same disappointment—"not yet arrived; must be published irregularly, or perhaps it is dying." Now I think it fills a great want, and would be sorry if it ceased to exist, although I myself will be obliged to give up, as I cannot go on calling at the bookseller's eight, nine, and ten times for each number.

However, wishing Knowledge all success, as it deserves to succeed,—Yours, &c.,

H. Armstrong.

[We regret that our correspondent should have been so badly treated. Not knowing either his bookseller or the bookseller's London agents, we cannot tell where the fault lies. One or other must have neglected to do what he was asked to do. Knowledge is published exceptionally early, and with perfect regularity. So far from dying, it thrives amazingly.—Ed.]

THE EQUALITY OF THE SOLAR ILLUMINATION THROUGHOUT THE SYSTEM.

[135]—I have lately fallen in with a rather curious theory concerning the "Solar Illumination throughout the System." Its originator appears to be perfectly satisfied that he has completely disposed of the long-accepted law of inverse squares, and seems to think that he has conclusively demonstrated that the sun's light is equally intense throughout the system. According to his theory, scientific men have for ages been labouring under a great delusion. Their idea that light decreases as the square of the distance, is a mistake. In our atmosphere, he admits, there is a decrease with the distance; but this, he affirms, is due to absorption by the air, and he is prepared to prove (to his own satisfaction, at any rate) that in space, where there is no absorbing medium, light does not diminish as the square of the distance. This theory is so bold and startling, that I should like to have the opinion of the readers of Knowledge concerning it. For my part, the arguments the author of the theory (Mr. Collyns Simon) uses are not conclusive enough to warrant our rejection of long and firmly-established principles. The admission of the truth of the theory necessitates a number of absurd consequences. The stars should appear to us as bright as our sun. And if the sun's *light* follows this law, so also must his radiant *heat*, which amounts to saying that the sun's heat would remain constant, even although it were possible to approach to his surface. Hoping to hear other of your readers on the subject,—I am, &c.,

An Admirer of Knowledge.

[Our correspondent is right in rejecting the theory of Mr. Collyns Simon. It is utterly untenable. I have had many letters from him, as well as books and pamphlets. Analysing his case, I find he misunderstands the evidence which leads to the sound theory, that apart from absorption, a luminous body presents a surface of equal apparent brightness from whatever distance it may be viewed—so long as it presents a visible surface at all. If we receded from the sun till our distance was twice as great as it is, his disc would look just as bright, but only one-fourth as large, as it does. The correct inference is that we should get but one-fourth of the light we actually receive. But somehow Mr. Simon makes out that we should get quite as much as we do at present.—Ed.]

[Note.—We have been compelled to omit the Chess Column for this week, owing to the pressure of correspondence on the one hand, and the necessity of assigning a certain portion of our space to original matter on the other. Our correspondents must not be surprised at the compression in many cases, and the omission in others, of letters, many of which we should have been glad to publish in full, had space permitted. A long letter from our esteemed correspondent, "A Fellow of the Royal Astronomical Society," is deferred till next week. It contains answers to many of the questions which have been asked by correspondents.—Ed.]

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Queries.

[92]—Falling Bodies.—Would a body, let fall from a height fall directly towards the centre of the earth or not? If not, would deviation be caused

(*a*) by the attraction of neighbouring bodies,
(*b*) by centrifugal force,
(*c*) by the velocity imparted by rotation round the earth's axis; or,
(*d*) by a combination of these causes?

Would not a body in the northern hemisphere fall to the S.E.; and one in the southern hemisphere to the N.E.?—$\frac{W}{g.}$ [We have had greatly to shorten $\frac{W}{g.}$'s question; but the above gives its purport. We answer it ourselves, to save space, and also the trouble of selecting between several answers. First, then, cause (*a*) is usually very slight, but in the case of a mountain or other great irregularity of contour or of internal structure of the earth's crust below the point of suspension, would produce a slight deviation from the vertical; (*b*) and (*c*) really relate to the same cause of deviation, which operates everywhere except at the pole. If h be the height of the point of suspenion above the surface, and r the earth's radius, then before falling, the body has a velocity due to rotation which bears to the velocity of the point on the earth's surface vertically below it, the ratio $(h \times r) : r$. Owing to this difference of velocities, the course of the body as it falls is not directly towards the point below but is, such as to carry the body somewhat to the east of that point (in both hemispheres). There would be no deviation north or south in either hemisphere, because a plane through the earth's centre and the point of suspension, and tangent to the circle in which this point is carried round the earth's axis, cuts the earth's surface at the point below the point of suspension in an east-and-west line. All the motions which affect the falling body, considered with reference to the earth, take place, during the fall, in this plane, and therefore cannot cause the body to leave that plane or to deviate from the east-and-west line. If the time of fall be t, the earth's rotation period P, height of the point of suspension h, and latitude of the place λ, the easterly deviation will be $2\pi h \frac{t}{P} \cos \lambda$.—Ed.]

[93]—Water of Ayr Stone.—I should be glad if any reader of Knowledge would tell me how water of Ayr stone is cut into slips $\frac{3}{4}$ in. by $1\frac{1}{4}$ in. by 6 in.? They have the appearance of being sawn with a circular saw. If they are, is the saw blunt, or with teeth; if with teeth, coarse or fine cut? Also if sand or water is used in cutting, or both?—T. G. H.

[94]—Watch Pivots.—How is arbor held and revolved in turns to reduce a pivot? Also how to proceed in renewing a broken pivot?—T. G. H.

[95]—Watch Jewel.—What tools are used in putting in new jewels, and how to proceed?—T. G. H.

[96]—The Earth's Centre.—If pressure (or weight) at the surface of the earth depends on gravity, then at the centre of gravity of the earth this attraction will be upwards in every direction, and weight will consequently be nil. The region of greatest pressure will thus be situated somewhere between the centre and the surface. Will this have any influence on the relative densities of the earth's interior; and will not the matter at the centre be "light as air?"—J. A. L. R. [J. A. L. R. confounds pressure with attraction. Attraction vanishes at the centre, but the pressure, which results from the weight of all matter between the centre and the surface, is there at a maximum.—Ed.]

[97]—We know the inclination of the earth's axis produces the change of seasons, and that the northern winter occurs at perigee, and the southern winter at apogee. Also, that in about 12,000 years this condition of things will be reversed, and the northern hemisphere will then be most favourable to a glacial epoch, which undoubtedly the southern hemisphere now experiences. Is there any significance (bearing in mind the preceding statements) in the fact that the land predominates in the northern and the water in the southern hemisphere? And may we assume that when the climatic conditions of the two hemispheres change, the physical features likewise will change, and the maximum of water then be in the northern hemisphere? Or is it a mere coincidence?—J. A. L. R. [A French student of science, Adhèmar, started the theory that the relation referred to by "J. A. L. R." is not a mere coincidence, but there is no evidence to show that the hemisphere where there is most water is necessarily that where the earth's nearest approach to the sun occurs in the summer time. The changes in the eccentricity of the earth's orbit, and the position of the place of nearest approach, are not quite so regular as Adhèmar supposed. See my article "Astronomy," in the "Encyclopædia Britannica," pp. 795 and 796.—Ed.]

[98]—Kindly inform me when Mars will be at its next best point for telescopic observation, and the simplest method for arriving at the result.—Algol. [On Dec. 27 Mars will be most favourably situated for observation. What result do you mean?—Ed.]

[99]—Summer Days and Winter Nights.—My almanack tells me the sun reaches its most northern declination of 23° 27′ 14″ on June 21, when it rises at 3 h. 45 m. and sets at 8 h. 18 m.; that on Dec. 21 the declination south is 23° 27′ 12″, sunrise at 8 h. 6 m., sunset 3 h. 51 m. How is it, if (as I have always understood) the condition of things is exactly reversed on these days, that the length of the *longest* day and *longest* night are not equal? Why does not the winter day commence at 8 h. 18 m., the time the summer night begins? The June day has a duration of 16 h. 36 m., the December night a length of 16 h. 12 m. How is this?—F. F. [The difference is due to the refractive action of the air, which causes the sun to appear to rise sooner than he actually does, and to set later. This causes every day to be longer than it otherwise would. The actual interval between sunrise and sunset (for sun's centre) would be 16 h. 24 m. on June 21, and this would also be the interval between sunset and sunrise on Dec. 21, were it not for refraction.—Ed.]

[100]—Stings of Wasps, &c.—How do you account for the stings of hornets, bees, and wasps not affecting the toad when it eats them?—Ornithorynchus.

[101]—Industry of Ants.—Are ants in reality the models of industry which they are proverbially reputed to be?—Ornithorynchus.

[102]—The Unseen Universe.—Can you refer me to any book or lecture of Professor Tait, of Edinburgh (I think it was Tait, not Crum Brown), in which the wonderful theory was expounded, as part of the idea of conservation of energy, that our words and actions, whether good or bad, come not to an end on the spot, but, converted into some form of indestructible energy, wander away into space, there, in some passage of eternity, in their accumulation, to work for the good or ill of the producer? I am so certain that I have seen something like this from Tait, a sort of scientific demonstration of the possibilities of a future state, that an early reply in Knowledge would much oblige.—A. A. F. [Probably the "Unseen Universe," by Tait and Stewart, is the work A. A. F. requires.—Ed.]

[103]—Could you recommend me a good book on "Histology" (of animals), and one on the Undulatory Theory of Light, treated mathematically?—D'Artagnan.

Replies to Queries.

[29]—Garden Tripod for Telescope.—Let the top of the tripod be a circle, 4 in. diameter, and 1 in. thick; into this the brass leg which the telescope probably now has may be screwed, or it may pass through and be clamped at any height, preferably the last, but I could scarcely explain it without an illustration. The legs should taper from 2 in. square to $1\frac{1}{4}$ in., of whatever length you require, so that they touch the ground about 4 ft. apart. The upper ends must be bevelled, so as to fit accurately against the circular top, and had better be hinged on the inner side, that they may fold inwards when not in use. The whole must be firmly made, or it will be useless.—Paugul.

[30]—Solar Heat.—(1.) The difference in distance is as nothing compared with the distance of the sun. Any physical geography will explain that it is because of the less vertical direction of the sun's rays. (2.) Both the sun's direct action, and the reflection of his rays from bright objects, snow, sand, &c. Radiation from the earth plays no part in the matter.—Paugul.

[41]—The Planet Vulcan.—I don't believe that Lescarbault ever saw an intra-mercurial planet, but it appears that not only one, but three or four have been seen.—Paugul. [The existence of an intra-Mercurial planet is very doubtful.—Ed.]

[42]—B.Sc. and D.Sc., Lond.—Surely this "Enthusiast" has no conception of the vast store of learning for which he is asking when he speaks of the questions set in the D. Sc. Exam., Lond. If he intends to graduate in science, he should study at a university. If he will write to me, I will give him a long list of works which will cover a portion of the syllabus he mentions.—Paugul.

[50]—Wasted Energy?—The light which is absorbed is converted into heat, and raises the temperature of the body which receives it.—Alfred J. Shilton.

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[52]—Microscope.—One of the kind called medical microscopes, having the body-tube sliding in the stand, and in two parts. The objectives should be made to slide into position and not to screw—it saves time. This microscope permits a more rapid change of amplification, while still giving all other facilities for work as well as any other.—Paugul.

[54]—Chemical Questions.—The explanation of (1) given by C. T. B. is very good, but he is wrong in supposing that hydroferrocyanic acid has not been isolated. Mr. Theophilus Pitt accuses "Castor and Pollux" of being "inattentive" and "careless," adjectives which are very applicable to himself, as he has misread the question, or else he is entirely ignorant of the fact that ammonia and hydrosulphuric acid form ammonium sulphide; and he apparently thinks that ferrocyanide of potassium should give a precipitate with ammonia and ammonium sulphide, which is a serious mistake. With regard to question (2), C. T. B. suggests that the solution might be alkaline. How can a solution of potassium bisulphate be alkaline? The true explanation is that tartaric acid is unable to displace sulphuric acid from potassium bisulphate, although it can take up one equivalent of potassium from the neutral sulphate (K_2SO_4), forming bisulphate ($KHSO_4$), and bitartrate ($KHC_4H_4O_6$). Potassium iodide should give a precipitate if the solutions are strong. The tartaric solution ought to be very strong (1 part in 4 of water). A solution of platinum tetrachloride is a far more delicate and reliable test for potassium; it should be used in the same way as tartaric acid, and, like it, also precipitates ammonium salts.—E. F. H.

[55]—Tables of Meridional Parts.—The meridional parts inserted in most works on navigation, as in Norie, Bowditch, Inman, Raper, Chambers, &c., are very old, and were computed for compression 0, in fact, for the earth as a sphere; these differ among themselves only in so far as Chambers and Inman give the quantities to two places of decimals. Mendoza Rios used compression $\frac{1}{321}$, and these are the tables given by Riddle. Rümker used compression $\frac{1}{303}$, and are the same as given by Caillet; but, by far the best table of meridional parts is that given in the "Projection Tables" computed for the United States Coast Survey Department and Hydrographic Office, and reprinted in England in the last edition of Raper's "Nautical and Logarithmic Tables"; the compression is here taken as $\frac{1}{299{\cdot}1528}$, based on Bessel's determination.—W. H. R.

[56]—Roscoe and Schorlemmer's "Treatise on Chemistry" (Macmillan).—Alfred J. Shilton.

[58]—Sirius and Orion.—The statement was, of course, wrong. Betelgeux must have been meant.—Paugul.

[62]—Algol.—Perseus is represented as holding the head of Medusa; the stars he mentions are one.—Paugul.

[63]—Algol and Mira.—Mira was at its maximum early last July, and will be a month earlier next year. Algol every couple of days.—Paugul.

[65]—New Star in Cassiopeia.—We do not know. A star has been seen, or is said to have been seen, two or three times in or near Cassiopeia, at such dates that, if correct, it is nearly due now. That is nearly all we are able to say; a small star has certainly been seen lately in Cassiopeia where one was not noticed before, but it may have nothing to do with the supposed variable.—Paugul.

[77]—Horrible dreams are often due to the quantity and quality of food taken at supper. I think Pliny the younger tells you to eat heartily of roast pork just before retiring if you want horrid dreams. The worst dream I ever had was due to pickled cabbage and cheese just before going to bed. The results are thought to be due to pressure on the solar plexus of nerves in the abdomen, which set up some irritant action in the brain. To avoid bad dreams, eat light suppers, at least three hours before retiring; take a little gentle exercise if possible, and avoid startling romances at night.—T. R. Allinson, L.R.C.P.

[79]—Mental Physiology.—"S. S. S. S." should have stated how far he wished for works of a "specialist" physiological character. Outside these, I don't think he can have a later, or a better or more complete work on the *mental* side of the subject (and which gives references to all needful works) than "An Inquiry into the Process of Human Experience, &c.," by William Cyples, published by Strahan & Co., 34, Paternoster-row, price 21s.—S.

[79]—Mental Physiology.—Dr. Carpenter's "Mental Physiology" is a capital book. "S. S. S. S." should look over the list of the "International Science Series" (Kegan Paul & Co.) for several works on the brain and mind. Dr. Maudsley's books on mental subjects will also be found very useful by a student of mental science.—Andrew Wilson.

[83]—In answer to (*a*), Berthelot describes the powdery form of sulphur in vol. xlix. of "Annales de Chimie." It is formed also at the zincode of a battery, in the electrolysis of sulphurous and sulphuric acids. (*b*) Nitric oxide in meant.—C. T. B.

[83]—Sulphur Modification.—There are five modifications of sulphur known. The two crystalline forms, the plastic form, a dark brown powder left on treating the plastic form with CS_2, and a light yellow powder insoluble in CS_2 (carbon disulphide) deposited from solutions of sulphur in CS_2, on being allowed to stand. The above is from "Roscoe and Schorlemmer's Chemistry," Vol. I., paragraph 156. (C.) It should be nitric oxide.—A Greenwich Student.

[84]—Ancient Man.—"Clio" (Query 84, p. 123) desires some information as to the reliability of the researches of Professor Horner. The Professor published them in 1854. He states that he made ninety-five vertical borings in the alluvial deposits of the Nile valley, and at the depth of 39 ft. fragments of pottery were found, and the conclusion drawn is, to use the Professor's own words, "if there is no fallacy in my reckoning, these fragments of pottery must be held to be a record of the existence of man 13,000 years ago." The whole data of his conclusion rests upon the assumption that the deposit of the Delta was gradual and uniform. This he assumes from calculating the interval between B.C. 1450, the time of the erection of the statue of Rameses II., and the time at which the exploration took place. And judging from the thickness of the deposits around the statue, from its base upwards, he infers that the Delta accumulated, if my memory serves me, at the rate of two and a-half inches a century. It will be perceived that this calculation takes as its basis that the accumulation was successive; but we have no means of ascertaining how far the base of the statue stood above the reach of the inundation when first put up, and so have no basis for any calculation. Again, the water of the inundation having been for ages kept out, according to Egyptian custom, from the enclosure in which the statue stood, the accumulation of the deposit there was the more rapid when, in after times, the water was admitted. This accounts for the thickness of the sediment without any *successive* deposit. Herodotus (Book ii., p. 99) mentions that Menes, first king of Egypt, B.C. 2350, was supposed to have diverted the course of the Nile by a dam twelve miles south of Memphis, and thus to have dried up the old bed. It may be that the statue of Rameses II. stands on the old bed, and the fragments of pottery were deposited by after inundations. Less than a thousand years ago the Nile flowed close to the western shore of Cairo. It is now separated from it by a plain more than a mile in width. At a depth of 30 ft., fragments of pottery were found less than a thousand years old. Professor Horner says that "fragments of burnt brick and pottery were brought up from the lowest part, viz., 50 ft. from the surface." Now it is an undoubted fact that there is not a single structure of burnt brick from one end of Egypt to the other, earlier than the Roman dominion. Mr. Birch, the Egyptologist, refers the burnt brick to B.C. 1300. On these and other grounds I think it may safely be said that the evidence for man's existence as derived from the researches of Professor Horner is not reliable.—R. S. Couch.

[88]—Brain Injuries.—The brain is divided into two parts. One controls involuntary motion, as breathing, the heart's action, &c.; the other is used for thought, the reasoning powers, and passions. The brain is also double, so that, injure one part, and the other side takes up its action. This is true with the reasoning and thinking powers. Injure a part governing any member, and you see at once paralysis of that member. From this is inferred that injury to the intellectual part is not followed by any noticeable change, but injure any of the motor parts, and you get at once paralysis. The brain is the organ of feeling, in that it makes us aware of injuries inflicted anywhere on the body: so that we make an effort to remove the injured part from the irritant.—T. R. Allinson, L.R.C.P., &c., 2, Kingsland-road, E.

[90]—The best theory is, that the earth is an electro-magnet; currents of electricity travelling round at about right angles to the axis. It is based on the fact that lodes of metals generally lie in the same direction, and they would be able to keep up thermo-electric currents at the expense of the earth's heat. This theory does not explain magnetic variation.—C. T. B.

[105]—A Question of Grammar.—"A. T. C." will, perhaps, understand the phrases, "I can but think" and "I cannot but think," by remembering that the word "but" is used in three senses:—(1) As an adverb = "only," as in "I can but think"; (2) as a conjunction, its most common use; (3) as a preposition = "except," as in "I cannot but think"—this expression being elliptical, for "I cannot do any other thing but think." In this last case (which, by-the-bye, some older grammarians ignore) "but" is the representative of the Anglo-Saxon *butan* = without; and is used by Chaucer in the lines:—

"But meat or drinke, she dressed her to lie
In a dark corner of the house alone."

The broad Yorkshire dialect has such expressions as "I can do bout it"; where the "bout" is not merely a corruption of "without," but the old Anglo-Saxon "but."—J. H. L.

XUM

Our Mathematical Column.

THE WITCH OF AGNESI.

[Reply to E. H. R., Mathematical Query, 2.]—The Witch of Agnesi is a curve of the third order described in the "Analytical Institutes" of Maria Gaetana Agnesi (1748). It may be thus drawn geometrically:—

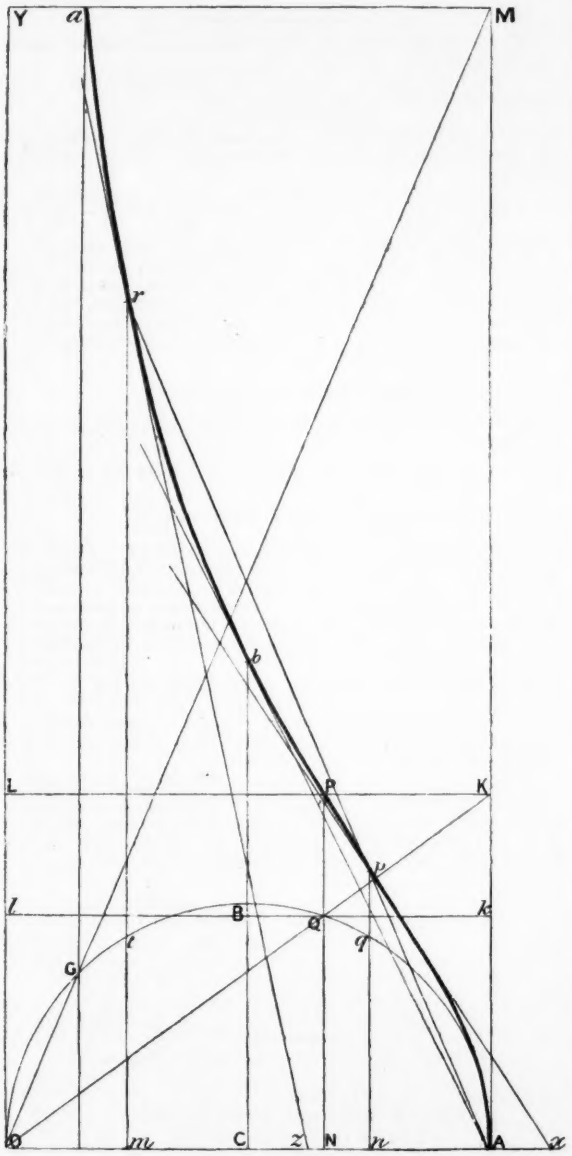


Fig. 1.

Take a circle OBA and draw a diameter OA; then if QN perpendicular to OA be produced to the point P, such that

$$NP : NQ :: AO : NO$$

P is a point on the "Witch of Agnesi."

It is evident that the curve will extend indefinitely on either side of OA (we only show one branch); and that the tangent to the circle ABO, at O, will be an asymptote to both branches. Again, if C be the centre of the circle; OC, CA, bisected in m and n; and BC, qn, and tm, perpendicular to OA, are produced to meet the curve in b, p, and r, it is obvious that

$$bC = OA = 2a \text{ (putting } OC = a)$$

$$pn = \frac{OA}{On}.\ qn = \frac{4}{3}qn = \frac{2}{3}\sqrt{3a}$$

$$rm = \frac{OA}{Om}.\ tm = 4tm = 2\sqrt{3a} = 3pn$$

Thus, a straight line drawn from A to p and produced will pass through r.

Again, if we draw AkM a tangent at A and LPK, lQk parallel to OA, it follows from the fundamental property of the curve that

$$\text{rect. } LN = \text{rect. } lA,$$

whence Q is on the diagonal OK. Thus, we have a simple geometrical method of drawing the curve, as follows:—

On AM take any point K, and join OK cutting circle OBA in Q; then parallels KP, QP to OA and OY, intersect in P a point on the curve. (We give the construction for the point a on the curve, joining MO, which cuts OBA in G, and drawing the parallels Ma, Ga.)

For the equation to the curve, take OA as axis of x, and OL as axis of y. Then, we have—

$$ON.PN = OA.QN; \text{ that is}$$

$$xy = 2a\sqrt{2ax - x^2}; \text{ or } xy^2 = 4a^2(2a - x).$$

From this equation to rectangular co-ordinates we can deduce the properties of the curve. Differentiating—

$$\frac{dy}{dx} = -\frac{2a^2}{x}(2ax - x^2)^{-\frac{1}{2}} \qquad \text{(i)}$$

and

$$\frac{d^2y}{dx^2} = \frac{2a^2}{x^2}(3a - 2x)(2ax - x^2)^{-\frac{3}{2}} \qquad \text{(ii)}$$

From (ii) we see that $\frac{d^2y}{dx^2}$ disappears when $3a - 2x = 0$, or $x = \frac{3a}{2}$; so that there is a point of inflection at p.

Putting x successively equal to $\frac{a}{2}$, a, and $\frac{3a}{2}$ in (i) we find:—

$$\frac{dy}{dx} \text{ at } pt.\ r = -4a\left(a^2 - \frac{a^2}{4}\right)^{-\frac{1}{2}} = -\frac{8}{\sqrt{3}}$$

$$\frac{dy}{dx} \text{ at } pt.\ b = -\frac{2a^2}{a}\left(2a^2 - a^2\right)^{-\frac{1}{2}} = -2$$

$$\frac{dy}{dx} \text{ at } pt.\ p = -\frac{4a}{3}\left(3a^2 - \frac{9a^2}{4}\right)^{-\frac{1}{2}} = -\frac{8}{3\sqrt{3}}$$

From these values, remembering that

$$rm = 2\sqrt{3}a;\ bC = 2a;\ \text{and } pn = \frac{2a}{\sqrt{3}},$$

we see that the tangent at r cuts OA at a point, z, such that $mz = \frac{3a}{4}$; the tangent at b passes through A; and the tangent at p cuts OA at a point x such that $nx = \frac{3a}{4}$.

A geometrical construction for finding the point x in which a tangent at a point p cuts the axis of x is easily obtained. For we have $\frac{dy}{dx} = -\frac{2a^2}{x}(2ax - x^2)^{-\frac{1}{2}}$

whence since $qn = \sqrt{2ax - x^2}$ it follows that

$$\frac{pn}{nx} = \frac{OA.OC}{On.\ qn^2}, \text{ or } \frac{pn.On}{nx} = \frac{OA.OC}{qn}$$

$$i.e.\ \frac{OA.qn}{nx} = \frac{OA.OC}{qn};\ \text{or } (qn)^2 = OC.nx.$$

Hence, if we take $nm = OC$, a perpendicular at q to mq will cut OA (produced, if necessary) in the required point x, such that xp is a tangent at p.

We have not by us any treatise bearing on the history of this curve, and are, therefore, unable at the moment to state the properties which led to its invention. In Brande's "Dictionary of Science and Art" the definition of the curve is given, and we have based on that the above investigation. Most probably the curve was one of those which mathematicians were fond of inventing (in the 17th and 18th centuries) for the purpose of squaring the circle. (Such a curve they would call a quadratrix.) It is manifest that the curve can readily be swept out mechanically. Thus, let there be a semicircular groove OBA (Fig. 2) and a straight groove OT square to OA, and let bars Nn', Ll slide between the parallels AT, Oa (square to OA) so as always to be parallel to AT and OA respectively. Then if a rod OG swinging around O, carry points along AT and ABO, which respectively bear with them the parallels Ll, $n'N$, a pencil so carried as to lie always at the intersection of these parallels, will trace out the curve APb.

It is readily seen that the curve is a quadratrix. For, take any two points, K,J, near together on OA, and about O as centre take circular arcs Kq', Jq to the semicircle AqO. Draw OqL, $Oq'L'$, as in fig. 2, and parallels $L'P'l'$, LPl, $mP'q'M$, $n'PqN$, giving two points, P', P, on the curve. Draw qn perp. to mM. Let $Oq\ Ll$, cut mM in s' and s respectively, arc $q'K$ cutting Oq in o. Draw the quadrant Ara about

O as centre, cutting OL, OL' in r, r' respectively, and join Jr, Kr' which are obviously perp. to OA (triangle AqO equal in all respects to triangle rJO, &c.) Then triangle $e'q'q$ is in the limit obviously isosceles, $q'o$ perp. to qe', and $e'q = 2oq = 2KJ$.

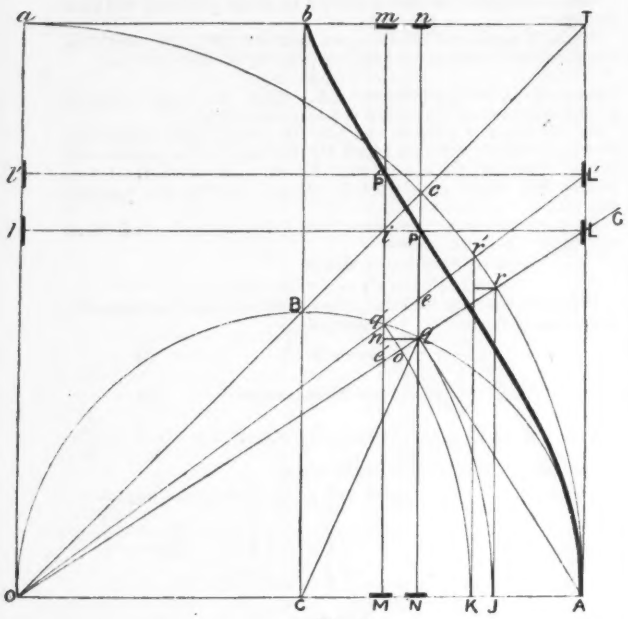


Fig. 2.

Now area $PM = PN.\ qn = qN.\ \frac{OA}{ON}\ e'q.\ \cos.\ nqe'$

$= qN.\ \frac{OA}{ON}.\ 2KJ.\ \frac{ON}{Oq} = 2qN.\ \frac{OA}{Oq}.\ KJ$

$= 2qA.\ KJ = 2rJ.\ KJ = 2$ rect. rK

This being true of all such areas, as OL passes from OA to OL and onwards, it follows (proceeding to the limit) that

area PNA = 2 area rJA

and entire space between OA, Oa produced indefinitely, and the curve APb carried on indefinitely

= 2 quadrant $AraO$

= 4 semicircle OBA

Therefore, entire area between the "Witch of Agnesi" and its asymptote is equal to four times the area of the circle from which the curve is formed in accordance with its definition.

It may be noticed that we can thus (after a fashion) square the circle, without carrying on the curve to infinity. For, let CB perp. to OA meet the witch in b, and let OB produced meet the quadrant in c. Then we know that

Area $APbC$ = 2 area AcN.

= 2 (sector AOc − triangle cON).

= quadrant AOa − triangle OAT.

= circle ABO − its inscribed square.

$= \pi a^2 - a^2$.

So that if $APbC$ be regarded as determined by this mechanically constructed curve, $\pi a^2 - a^2$ is determined, too; or since a^2 is known πa^2 the area of the circle OBA is determined.

In passing, it may be noted that from the above construction follows a ready demonstration of the property of the tangent, established analytically above. For the limit PP' is a straight line, and the tangent of the angle which PP' makes with OA is ultimately the $\frac{dy}{dx}$ of our analysis.

Now tan. $P'PL = -\frac{P'i}{Pi} = -\frac{LL'}{nq}$

$= -\frac{q'e'}{nq}.\ \frac{OL}{Oq} = -\frac{q'q}{nq}.\ \frac{OL}{Oq}$

But—

$\frac{PN}{qN} = \frac{OA}{ON} = \frac{OL}{Oq}$

$\therefore$ tan. $P'PL = -\frac{q'q}{nq}.\ \frac{PN}{qN} = -\frac{Cq}{qN}.\ \frac{PN}{qN}$

$= -\frac{OC}{(qN)^2}.PN$

This is the same relation as we have already obtained; for if a tangent at P meets AO produced, in a point x, we have

tan. $P'PL = -\frac{PN}{Nx} = \frac{OC}{(qN)^2}PN$ as just shown.

So that $Nx = -\frac{(qN)^2}{OC}$ Ed.]

"G. P." considers that he has solved the very difficult simultaneous equations

$x^2 + yz = a^2$ (i) $\quad y^2 + zn = b^2$ (ii) $\quad z^2 + xy = c^2$ (iii)

by showing, what is in truth obvious, that

$x^3 - a^2x = y^3 - b^2y = z^3 - c^2z$.

"It is now reduced to a cubic" (we fail to see this) "which might be solved in a variety of ways. For instance, if we suppose $x = ny$, we get:—

$$y = \sqrt{\frac{a^2n - b^2}{n^3 - 1}};\ x = n\sqrt{\frac{a^2n - b^2}{n^3 - 1}};\ z = \sqrt{c^2 = n\left(\frac{a^2n^2 - b^2}{n^3 - 1}\right)}$$

(Where we have written b^2, "G. P." writes 1, through some error.) From which by substituting in (i) (ii) and (iii), we find n, and hence x, y, and z. It is evident G. P. has not tried substituting in (i) (ii) and (iii). If he had, he would have found, first, that substitution in (i) or (ii) would suffice [(iii) giving only an identity], and that the resulting equation in n would be of the 6th degree!

MATHEMATICAL QUERIES.

[5]—Differential Calculus.—"G. P." next asks why, if we take the relation

sin. $x = \frac{2-x}{2+x}$ and differentiate with respect to x, we get

cos. $x = -\frac{4}{(2+x)^2}$ instead of cos. $x = \frac{2\sqrt{2x}}{2+x}$

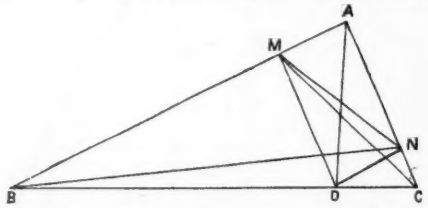
as by trigonometry. "Whence the contradiction?" Simply because "G. P." has applied the differential calculus the wrong way. His relation assigns a definite value to x and to sin x, and that being so, we cannot differentiate. Take a simpler relation, and we may find the differential calculus, applied in this way, contradicting common sense. Thus take $x = 1$, and differentiate both sides with respect to x, and we get $1 = 0$.

[6]—Indeterminate Equation.—Is it possible to solve

$x = 12 + 2y$?

to find the value of x and y, without *assuming* any numerical value. —J. A. L. R. [There must always be some assumptions in the solution of indeterminate equations.—Ed.]

[7]—Could you kindly favour me with a solution to the following rider on II. Book of "Euclid":—"The angle A of a triangle ABC is a right angle, and D is the foot of the perpendicular from A on BC; DM, DN, are perpendicular on AB, AC, respectively: show that the angles BMC, BNC are equal."—Nemo Impune.



[As a rider on Book VI., thus:—

Triangle BAC similar to triangle AND,

$\therefore$ BA : AC :: AN : ND (=AM).

Hence BA. AM = AC. AN, wherefore points B, M, N, C, lie on circumference of a circle,

$\therefore$ angle BMC = angle BNC.

But the problem can be as readily solved without the aid of any properties beyond those in Book III., thus:—Join MN. Then, triangle AMN being in all respects equal to the triangle NDA, the angle AMN is equal to the angle ADN, and therefore to the angle ACD (ACD and ADN being obviously equal, because each with the angle DAN makes up a right angle). Hence the angles BMN and BCN together are equal to the angles BMN and AMN together. or to two right angles. Therefore B, M, N, C are points on the circumferenco of a circle, and therefore as above

Angle BMC = angle BNC. —Ed.]

Our Whist Column.

FORCING AT WHIST.

Sir,—I gather from the character of your published articles that you presuppose in your readers some knowledge of the theory of whist. So much, in fact, has been written upon whist that it is difficult to find anybody absolutely ignorant of its principles. It is of great value, however, to the young player, to place before him in a condensed form the digested experience of the best players, and thus raise him gradually from domestic to scientific play. "When may I force my partner?" is a question frequently put. There are, undoubtedly, many positions in practice where the thoughtful but inexperienced player finds himself in difficulty. It is easy enough to understand the reasonableness of forcing an adversary who has shown great strength in trumps, or a partner who has shown great weakness. But suppose, for example, as an original lead, a player were to lead from manifest weakness, an honour having been turned to his right, that which in ordinary cases appears to be an invitation for a force, would, in fact, amount almost to a direction to lead through the honour. But I will endeavour to lay down the cases when a player, not having trump-strength, may, nevertheless, force his partner:—(*a*) When, with no indication of strength, he asks for a force. (*b*) When the position shows a cross-ruff. (*c*) When the adversaries have signalled. (*d*) To make the fifth or odd trick, or to save the game, when the hand of the forcing player, or the development of the game, does not raise a high degree of probability that the necessary trick may otherwise be made. But an interesting point relating to the force is where the player, in a position to force, has trump-strength amply justifying it. It often happens that a player renounces to the lead of his partner, who, with ample trump-strength, has no strength in the then declared suit. If he forces, and the declared suit be not headed by Ace King, or King Queen, the result is, after a force, a lead up to ruinous weakness. No trick is gained by the force, for another trick is lost in the suit. If, however, the player gives his partner his declared suit, the adversaries may infer that he has no strength in trumps, and lead trumps to their disadvantage. I send you a game illustrative of this position, and hope it may be interesting to your readers.

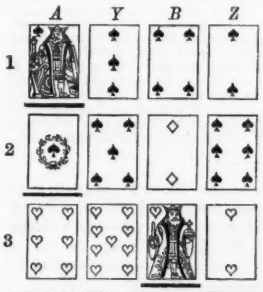
Frederic H. Lewis.

THE HANDS.

A.	B	Y.
Clubs—Q, 9, 4.		*Clubs*—Kn, 3, 2.
Hearts—6, 3.		Hearts—A, 10, 9.
Spades—A, K, 8, 7.	Dealer.	Spades—Q, 10, 5, 3.
Diamonds—Q, 5, 4.		Diamonds—A, K, 3.
	Z	
B.	*Trump Card, King of Clubs.*	Z.
Clubs—10, 7, 6.		*Clubs*—K, 8, 5.
Hearts—K, Kn, 7, 5, 4.		Hearts—Q, 8, 2.
Spades—4.	A	Spades—Kn, 9, 6, 2.
Diamonds—Kn, 10, 6, 2.		Diamonds—9, 8, 7.

Score:—A B = 1; Y Z = 4.

Note.—The underlined card wins trick, and card below it leads next.



INFERENCES.

1.—*A* leads from his strongest suit.

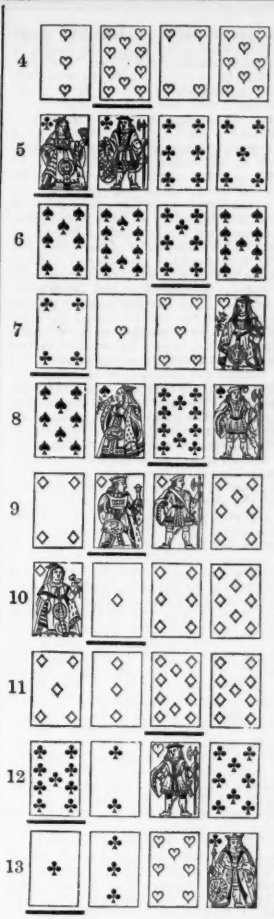
2.—The fall of the cards, and the discard of Two of Diamonds, the lowest of the suit, shows *A* that no one is signalling for trumps.

3.—*A*, having here two honours in trumps, might very well force his partner, but, having only Six and Three of Hearts, prefers to give his partner his suit.*

* If he had forced him, the game would, probably, have proceeded as follows:—

	A	*Y*	*B*	*Z*		*A*	*Y*	*B*	*Z*
3.	S 7	S 10	C 6	S 9	9.	H 6	H 10	H 4	H 8
4.	H 3	H 9	H 5	H 2	10.	C 4	H A	H 7	H Q
5.	D 4	D K	D 6	D 7	11.	C 9	C Kn	C 10	C 5
6.	D 5	D A	D 10	D 8	12.	C Q	C 2	H Kn	C 8
7.	D Q	D 3	D Kn	D 9	13.	C A	C 3	H K	C K
8.	S 8	S Q	C 7	S Kn					

and the result would be two, instead of four, by cards.



4.—*B*, although having no strength in trumps, is here quite justified in returning the Heart. If his partner has avoided forcing him, in consequence of weakness, the chances are that the game is lost. If, however, a cross-ruff can be secured, such a position would be most advantageous.

5.—*Y* sees that a cross-ruff must be secured. He has the tenace in Spades, the best Heart, an honour has been turned; he has the command in Diamonds, and *A* has avoided forcing *B*. All this is too much for weak human nature, and he cannot resist a trump lead; he therefore leads the highest of his three trumps.

6.—*A* is now in a position to force his partner advantageously.

7.—The cross-ruff: conveying also to *A's* mind, from the fall of the cards, that the two remaining Hearts are with *B*.

8.—*A* is in a position to give another force.

9.—*B* cannot lead one of his long Hearts, such play would be very bad, and accordingly he leads the best of his remaining Diamonds, treating the suit as though he had originally but three.

10.—*A* now has the tenace in trumps, but he requires three tricks to win the game. If he keeps the Queen of Diamonds he can make only two; he, therefore, cleverly throws the Queen, taking the chance of his partner having the Ten of Diamonds. The fact being so, he secures four by cards and the game. The rest of the hand plays itself.

Answers to Correspondents.

*** *All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule* 3, *free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

W. We cannot afford space for your long letter, containing only assertions without evidence. Newton waited nineteen years before advancing his theory of gravitation, because, though it agreed in all other respects with observed facts, it would not stand one test—the reason being that the accepted measurement of the earth's globe was incorrect. Cannot Mr. Crosland's friends allow his astronomy of the future to wait until it has been shown that it accounts in measure and number for at least one single observed fact? We promise to give all the space you ask for vague generalities, for the first demonstrated case of the kind.—H. J. Pearson. (1) Strictly speaking, the exact sciences are those which are based on mathematics, as optics, astronomy, &c.; but the term is usually extended to all sciences depending on observation and experiment, so that it includes chemistry, and other physical sciences not strictly exact. The term is not a well-chosen one. (2) It cannot be "demonstrated" that the nearest way from one point to another is the straight line joining the two points; but we show that the latter path is shorter than any path made up of straight lines, and since a series of very short straight lines may be made to approximate as nearly as we please to a curved line, this can at any rate be demonstrated, that a curved line from one point to another differs by an indefinitely small amount from a length which is greater to an assignable degree than the straight line joining the two points.—Thos. Smith, Jun. Phrenology is not so young that, were it really a science, it should be unable to take its own part. It is much older than spectroscopy, which can stand alone very well. We did not, however, mean that phrenology is absurd, when we spoke of the absurdities of phrenology; there are reasonable features as well as absurdities in it. Much may be said in favour of a rational phrenology. Gall and Spurzheim were both scientific observers, and both made useful scientific discoveries; but their system of mapping out the cranium has now no adherents among men of science.—W. J. M. We have ourselves seen and heard a dog which was said to speak; but one had to make believe a

great deal. Thus. if you said "Œufs" to him (he was a French dog) he would make a noise about as near to the sound of the word as the note of the cuckoo is to the word cuckoo, but no nearer. If one had been told he said "hurt," the noise would have done just as well for that word, or for half-a-dozen others. In some such way, doubtless, the dog "seen and heard by M. Leibnitz" could say, in German, tea, coffee, &c. We had a dog once which could sing, but that required a good deal of "make believe" too.—John Steele. Your explanation not necessary, but well received. Science no more questions the sun's heat than we question the heat of a fire which warms us.—Altair. Query 60 already answered. Thanks. As regards 62, Algol is shown in the maps for this month. It is scarcely a sufficient answer to 63 to note that Algol and Mira are brightest when at their maximum brilliancy. Note our reply to 64. —Q. E. F. Your own figure should have shown you that your solution is unsound, for the angle is obviously not trisected. — Ridiculus Mus. Your first result amounts to this, that if you have made a certain number of moves in the Fifteen Puzzle, you can come back to the position from which you started; your second, to this, that starting from a certain position and making any number of moves, you cannot work back to a certain other position, closely resembling, but not identical with, the former. But this does not deprive the puzzle of its interest as a mathematical enigma. You can hardly have proved "by numerous experiments" (only) that these results are invariable, for there are twenty billions of different positions which can be attained from one or other of your two starting positions. If you had, (that is, if you had worked at the puzzle ten times or so a day, during a life of some twenty thousand years) there would still remain the problem—pretty, though not difficult—of showing why these results can invariably be attained. Then there is the neat puzzle in every case, how to attain any assigned position from another of the same order, in the smallest number of moves. On the whole, it is as pretty a little puzzle, I think, as any invented since "Solitaire," which involves some very neat mathematical relations.—Chromo. Thanks; but questions sufficiently answered already.—M. E. Pendred. Your fears were expressed for others, but seemed to be yours. Anyhow, we know your letter would have raised most undesirable controversies.—F. H. F. The lunar illusion is not dependent on the vastness of the sun's distance. "M.'s" explanation was sound. The plane through the observer's eye and the horizontal sagitta due south passes through the east and west points on the horizon. Would print your letter did space permit.—H. Rayment. Hope shortly to insert your letter.—An Admirer. You are very good, but fear readers of Knowledge would not quite like it.—E. Burgess. Thanks for "Help to Chess Students." Crowded out at present.—Athos. Of course you are right in saying that the solar dark lines, as interpreted, prove the sun to be intensely hot. The argument would be above comprehension of the paradoxers, however.—Rob. S. Couch. Letter shall shortly appear, if possible.—G. W. Niven. Short notes on such tricks would suit well enough.—S. N. B. G. No space at present. Thanks for replies.—A. L. S. Thanks for account of lunar rainbow (why call it Iris Lunaris?). No room for it at present.—J. Fraser. Still hold the accepted theory of the tides. No earthly reason why the earth should fall into the moon. Your theory wholly untenable. —T. Thanks. —F. W. C. Cannot insert answers recommending instruments by particular makers. We are sure of your *bona-fides*; but others would claim right to do likewise with less worthy motives.—J. M. Many thanks; but the question had been answered sufficiently, though we could not find room at once for reply.—W. F. Denning. Thanks; your letter shall appear.—Carlo F. Johnson. Thanks; but we take replies respecting comets as part of subject we are discussing in articles—if only we could get a line or two in edgewise. Other questions answered.—H. G. Hollingworth. Will try to find room.—D'Artagnan. Airy's Essay on "Undulatory Theory of Light" (in his mathematical essays), Lloyd's, and Potter's treatises were what we used to read at Cambridge. —Inquirer. Moore's "Lalla Rookh" is not a recognised astronomical authority; still "half retired" (from the earth) would correspond nearly enough with time when Venus "looks loveliest."—S. N. W. You evidently have not yet grasped the meaning of the modern theory of evolution. Your remarks apply well to such theories as Lamarck's and others; but they are dead; why play Falstaff at Shrewsbury over them?—J. D. "As fast as we fix a telescope," you say, "the earth's motion ought to displace it." That is just what the earth's motion does. It is this motion that the driving clock of a telescope is devised to correct, by constantly carrying the clock the other way.—F. H. S., J. Gottfried, and others. Thanks; "Magic Squares" crowded out for the present.—L. Ferrers. Received; thanks.—Mabel W. L. Pressure of time was the cause of printer's error; but it was of the less importance, as Brewster's ideas were entirely incorrect. —S. H. F.—We prefer our "happy laughter" to be independent of comparatively "low replies." Your method of studying the luminary in question is unsound.— J. Gottfried. Thanks for your magic squares; the method shall appear. There are others yet.—Andrew Aitken. Your answer to Cymro's question is sufficiently sound, but we have not room for second solutions. The first was correct. The first part of answer to Query 48 is also omitted, as the trisection of a right angle is familiar to all students of geometry. The other solution is really the same as that on p. 117, but if our space were not so crowded we would give it. Thanks for your communication all the same. We must ask you to consider our position with regard to space. Note beginning of our letter in correspondence column, and reply to W. R. W. below.—James Shaw. Article on "Right-handedness" shall appear shortly.—Eclecticus. We give you alone this *nom de plume* now. Have been obliged to much shorten your letter.—W. H. R. Thanks, but longer letter on meridional or meridianal parts could not be placed. There is no Latin word *meridionalis*. *Meridianus* is the word. Albeit we leave such points to the printers.—W. T. Lynn. Thanks, but the exact time when Halley's comet was first observed in 1759 is not likely to interest the majority of our readers, and space considerations trouble us. We put in your first letter, because it involved a correction of our own statement. Otherwise, these questions of dates, priority, manner of observation, and so forth, seem of little importance. How often has the error about Tycho Brahe and the new star been repeated, and what harm has it done? We must not be microscopical in such matters.— Sirius. To save space, have been obliged to incorporate your letter in our own. —Alex. J. E. Your 3-square puzzle is very neat.—Musicus. Why want to learn how to "get in the way of talking" while you play the piano? Did you ever hear anyone do so who had the least feeling for music? None such can play well when *others* talk. Probably, it is as easy for a mere "performer" to talk while he plays as to talk while someone else is playing, and as difficult (rather as impossible) for a real musician to do one as the other. To anyone with the slightest particle of musical feeling, there can be nothing much more trying than to hear people talk while a noble air is nobly rendered, unless it be to hear a skilful executant talking while the notes of a noble air pass without life or expression under his agile but not musical fingers.—E. F. H. Thanks.—Herbert R. Weller. Unquestionably a meteor. Some meteors move, apparently, quite slowly, and the larger often have the cometic appearance you indicate.—H. A. S. "Puzzled's" problem is sufficiently explained. It is much older than the publication you mention, though, doubtless, new to him. Your other problem, depending on half a live man being equal to a man half dead is amusing, but would be out of place here.—K. J. P. Why will correspondents take so much care to make their communications too long for insertion?—A Biologist. Your reply to No. 52, if inserted, would lead to others being sent, which might read like advertisements. Thanks equally.—J. H. W. L. We will see when first half-yearly volume approaches completion. Thanks for getting new subscribers.—C. J. Chamberlin. Your suggested paper would range too much, we fear, over the ground covered by text-books of Acoustics.— F. J. Brodie. Correction inserted. Thanks. Should receive such notes as you suggest with pleasure. Brevity essential, you will understand.—J. F. S. Thanks for extracts; several we had already cut out, but correspondence crowded them out. Note letter from W. M. Williams, and consider the cheerful task of weeding out and boiling down such abstract reports.—Camille Flammarion. Thanks for your obliging offer, of which we gladly avail ourselves. Book safely to hand. Again thanks.—Edward P. Thompson. Thanks; yet the quotation would hardly add to our knowledge: it tells us only what everyone knows—that fancy often mimics reason.

But misjoining shapes
Wild work produces oft, and most in dreams
Ill matching words and deeds long past or late.

G. Cuttle. Your r ply to Query 90, No. 6, is hardly explanatory. The question really is why the earth acts as you say—justly enough—she does act. By-the-way, the magnetic poles are no longer where you say. Changes both in the declination and inclination all over the earth sufficiently prove that these poles have shifted largely since the northern was determined and the southern inferred. —Paugul. Many thanks for your replies; a few, as relating to queries already answered, we omit, space being crowded. The paradoxers think me too hard on them, instead of "too indulgent."—R. E. W. We omitted the third extract, but failed to omit the reference to it.— Rev. W. H. K. S. Your explanation of ice-yachts sailing faster than the wind is not available in many known cases, where the relative wind is less than the absolute wind, and the velocity of yacht exceeds that of either. The principle of the Rhine ferries is different. Thanks.—J. H. Lamprey. Unquestionably, the "Knowledge of the Ancients," would be full of interest for students of modern knowledge; and occasional papers on the subject might suit us. But we cannot, as yet, arrange for the systematic treatment of ancient knowledge here. If you send us a short paper you may rest assured it will receive due attention.— Rev. W. Houghton. Thanks for your interesting letter, which shall appear in full.—J. Taylor. Your suggestion attended to; see "Dates and Hours for Stars in November," in this column below.—W. R. W. Are you not a little unfair? There was, of course, less correspondence in No. 1, than later; it would not have been strange had there been none. In No. 6, the correspondence occupied too much space, but most of the letters were interesting. Two were specially written for us, and might have been put among the original articles, only their writers modestly suggested that the matter should be put among the correspondence. You must remember that we have to consider those who ask questions, or feel moved to make replies. You rather miscount the pages in No. 1 and No. 6, including the star map in one estimate and omitting it in the other. However, No. 6 was exceptionally overweighted with correspondence. Hereafter we must be more strict, though, as it is, we are rebuked for hard-heartedness.—R. G. We cannot undertake to give the accents of technical terms. Miss Kingsford's statements claim courage for fruit-eaters as well as strength. Roman and Spartan soldiers were not altogether wanting in pluck. Many cases are recorded of flesh-meat acting as an intoxicant, even to people in sound health. However, the Indians who gesticulated may have been intoxicated otherwise. The question whether it is the use of flesh-meat which has raised man above his fellow Primates is, of course, open to discussion. It scarcely seems probable, in the face of the admitted high intellectual and physical standing of many races which eat no flesh.— Rev. J. H. L. I have had very little to do with fixing the price of my books, and doubtless it is the same with other writers. I wish the publishers of books on science could find reason, in the increased number who take interest in science, to lower prices.—T. R. Allinson. Have slightly shortened your reply; space inexorable.—Arnold Ramsden. You can hardly do better than get Mädler's smaller map of the moon, or Webb's "Celestial Objects for Common Telescopes," in which there is an excellent map, based on Mädler's. Webb's book is about 10s.; Mädler's map alone, from the German publishers, would cost, I suppose, a shilling or so.—Thomas Thorpe. Your ingenious instrument to appear in early number,—J. J. F. The mistakes of T. P. (A. K. C.) corrected elsewhere. Thanks.—Thomas H. Bamford. It is an open secret that Dr. Robert Chambers was the author of the "Vestiges of Creation." It would only waste time to discuss the claims of Lord Byron's daughter. By the way, no one who has read Lyell's remarks on the book in his "Antiquity of Man," could for a moment suppose he was the author.— G. C. D. M. We perfectly agree with your remarks about corporal punishment for boys. The cases are very few indeed where due obedience cannot be obtained without such punishment. But we cannot quite hold with you, that at birth the predilections for evil and for good in a child are exactly equal. Children differ singularly. Certainly George Eliot's skill as a novel writer would not of itself show that her brain was abnormally developed, but her novel-writing was only the means by which her real brain power was shown. Pray excuse non-appearance of your letter in full. The subject has now been pretty fully treated, and readers are asking that more space be given to articles.—Lionel T. Waller. Hope to give weekly weather reports shortly, if space can be found.—Wiseap. Will see whether it can be done without injuring the maps. Doubt it.—A. J. Shilton, Not Castor and Pollux, and Others. Thanks. Query 54 sufficiently answered.—A Well-wisher, and others, repeat a request to which we have several times responded, that advertisements should be printed so that the bound volume need not contain them. This is already provided for: no advertisement page is now numbered so as to form part of the bound volume. Compare Nos. 1 and 2.—Ed.]

Dates and Hours for the Stars in November.—These are as follows in the first four numbers:—Oct. 30, at 10½ o'clock; Nov. 3, 10½ o'clock; Nov. 7, 10 o'clock Nov. 10, 9½ o'clock; Nov. 14, 9½ o'clock; Nov. 18, 9½ o'clock; Nov. 22, 9 o'clock Nov. 25, 8½ o'clock; Nov. 29, 8½ o'clock; and Dec. 3, at 9½ o'clock.

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